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THE PHILOSOPHICAL
HISTORY
AND
MEMOIRS
OF THE
Royal Academy of Sciences at *Paris* :

OR,

An ABRIDGMENT of all the PAPERS relating to *Natural Philoſophy*, which have been publiſh'd by the *Members* of that *Illuſtrious Society*.

With many Curious OBSERVATIONS relating to the Natural Hiſtory and Anatomy of Animals, *&c.*

Illuſtrated with COPPER-PLATES.

The Whole Tranſlated and Abridged,

By *JOHN MARTYN*, F.R.S.

Profeſſor of Botany in the Univerſity of *Cambridge* ;

AND

EPHRAIM CHAMBERS, F.R.S.

Author of the Univerſal Dictionary of Arts and Sciences.

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AN
ABRIDGMENT
OF THE
Philoſophical Memoirs
OF THE
ROYAL ACADEMY of SCIENCES at *Paris* for the Year 1710.

X. *Obſervations on the variation of the needle, with regard to Dr.* Halley's *chart: together with ſome geographical remarks made from ſea-journals, by M.* Deliſle *; *tranſlated by Mr.* Chambers.

FAther *Gouye* having communicated to me 8 ſeveral journals of pilots, who ſteered ſhips from *France* to *Newfoundland*, and the *American* iſlands, I extracted from them what appeared of moſt advantage to navigation and geography; and having the uſe likewiſe of two other journals, one of them kept by M. *Hebert*, his majeſty's envoy to the *Indies*, and the other by M. *Bigot de la Cante*, ſecond lieutenant on board the king's ſhip the *Sphere*, to the coaſts of *Guinea* and river *de la Plata*, I have thought proper to join all the obſervations together made in thoſe ſeveral voyages, becauſe all made nearly in the ſame time, *viz.* in the years 1706, 1707, 1708, and 1709.

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* July 16, 1710.

The obſervations of the variation of the needle are of late become ſo eſſential to navigation, that the pilots take all occaſions of making them; in effect, they cannot know the rumb in which they ſail, without firſt obſerving the variation; and when the weather will not allow them to obſerve it, they are obliged to compute it from the obſervations of other navigators about the ſame places. They even begin to make uſe of it for correcting their reckoning, and finding, in ſome meaſure, the longitude, when, either by themſelves, or others, they know the variation which ſhould be found in ſuch and ſuch places. Thus M. *Daumas*, firſt pilot of the *St. Louis*, diſcovered hereby, that he was paſſing the line more weſtward than his reckoning gave him, and in the latitude of the iſland *Cape Verd*, he perceived by the variation, that he was 30 leagues more weſtward than by his reckoning. He adds, that upon approaching the iſland of *Bourbon*, in the *Indian* ſea, finding the variation to be 21°—— north-weſt, he learned hereby, that he was to the weſtward of that line, when his reckoning could not tell him ſo much.

It would be needleſs here to relate the computed variations, our knowledge on this head is much too ſcanty to make a juſt computation; I ſhall here therefore confine myſelf to ſuch as have actually been obſerved, among which I ſhall always prefer thoſe obſerved by the amplitudes to thoſe by the azimuth; the former appearing ſurer, and leſs ſubject to error.

The longitudes I make uſe of are reckoned from the pike of *Teneriff*, according to the charts of *Pieter Goos*, which are thoſe commonly uſed by our late navigators, for ſetting off their courſe.

At 120 leagues from the coaſts of *France*, and 44°——¼ latitude, one of the pilots, in the year 1709, found the variation 8°—— north-weſt by the ſun's amplitude; and M. *Daumas*, in the ſame place a little before, found it the ſame. Dr. *Halley*'s chart makes it 6°——½.

At 45°——7′—— latitude, and 11°——31′—— longitude, he found the variation 6°——40—— where Dr. *Halley* makes it 6°——½.

At 45°——20′——latitude, and 358°—— 15′——longitude, he found the variation 11°—, where Dr. *Halley* only makes it 9.

Another pilot, ſetting out from *Rochel* for the *Cape Verd* iſlands in 1708, found at 35°—35′— latitude, and on the meridian of *Teneriff*, the variation to be 4°——35—— by the weſtern amplitude: Dr. *Halley* makes it 4°——

At 27°——58′——latitude, and 353°—— 40′——longitude, he found the variation 4°— 32′— where Dr. *Halley* makes it 2°——10′——

At 36°——lat. and 325°——46′——long. he found the variation 5°——8′—— where Dr. *Halley* makes it 3°——½.

Another pilot at 46°——50′—— latitude, and 230 leagues diſtance from *Rochel*, found the variation in 1709, to be 7°—50′— where Dr. *Halley* makes it 7°—30′——

Soon after at 260 leagues from *Rochel*, and 47°—latitude, he found the variation 8°—— where Dr. *Halley* likewiſe makes it 8.

At 33°——45′——lat. and 5°—— long. he found the variation 6°—— where Dr. *Halley* only makes it 3°——¾.

The firſt pilot of the *Marianne* at 43°—45′— lat. and 340°——46′——long. found the variation in 1709, to be 13°—— where Dr. *Halley* only makes it 8.

All theſe obſervations were made on this ſide the line of no variation, ſuppoſing them exact, the variations muſt have increaſed on this ſide the line, ſince the time of Dr. *Halley*'s chart; but leſs in one place than another.

M. *Bigot de la Cante*, in a voyage to the river *de la Plata* in 1707, found at 44°—45′—lat. and 52 leagues diſtance from *Cape Finiſterre*, the variation to be 7°——20′—north-weſt, where Dr. *Halley* only makes it 6°——15′; and the ſame variation he found in the 4 enſuing days, during which he made 60 leagues ſouth-weſtwards. In Dr. *Halley*'s chart, it is 6°—— all along this line.

At 7°——15′——lat. and 1°——50′—— long. he found the variation 2°——30′—— where Dr. *Halley* only makes it 50′——

In the road of *Judda*, on the *Guinea* coaſt, he found the variation in 1708, 8°—20′. M. *Des Marchais*, in 1705, found it 8°—. In the ſame place, Dr. *Halley* only makes it 5°——

In the eaſtern part of the iſland of *San-Thomas* under the line, he found it 11°—30′—— where Dr. *Halley* only makes it 5°—30——

From hence M. *Bigot* ſailed ſouth-eaſterly to 4°——ſouth lat. not far from the coaſts of *Congo*, from whence he always bore to the ſouth-weſt, and weſt-ſouth-weſt, as far as the mouth of the river *de la Plata*, where he arrived in *April*, 1708. This traverſe of 14 leagues is the fitteſt that could be thought of for examining the variations aſſigned by Dr. *Halley* in this ſea; and the more ſo, as it cuts all Dr. *Halley*'s lines almoſt perpendicularly.

Along this courſe, the variation, which was north-weſt, grew leſs and leſs every day, till having made 560 leagues, it entirely diſappeared, and henceforwards the variations were north-weſt. This line of no variation Dr. *Halley* makes 120 leagues more eaſterly, than M. *Bigot* found it, and makes a variation of 1°——30′——north-eaſt, where M. *Bigot* found none; ſo that we are not to wonder, if the variation, found by M. *Bigot* before his arrival at this line, were greater than that aſſigned by Dr. *Halley*, by 1 or 2°—— and ſometimes more.

In *April*, 1709, M. *Bigot* left the river *de la Plata*, and purſued much the ſame courſe as he had kept before, for the ſpace of about 800 leagues; but the obſervations he made in returning do not agree with thoſe made in going, tho' by eſtimation he thought himſelf in the ſame places, where he had obſerved before. The place where in going he found the variation 20′—— north-eaſt, appeared by his eſtimation 9°—more eaſterly, than that where he found the variation 26′ in his return, and the other places in proportion to their diſtance from the mouth of the river *de la Plata*, which probably ariſes from the waters of that huge river, which communicating its current to the ſea, by a mouth 30 leagues broad directed to the eaſt, may be ſuppoſed to retard the motion of the ſhip in going, and accelerate it in its returning.

From hence M. *Bigot* ſailed to *Martinico*, and in his paſſage thence to *France* in 1709, at 28°—30′—lat. and 316°—30′—long. found the variation 1°——30′—north-weſt, where Dr. *Halley* makes it 1°—north-eaſt; ſo that the variation has here changed from north-eaſt to north-weſt, and the line of no variation, which was eaſtwards of this place, has travelled to the weſtward thereof ſince Dr. *Halley*'s chart, if we may credit theſe obſervations.

At 32°—15'—lat. and 321°—45'—— long. he found the variation 4°——10', where Dr. *Halley* makes it 2°—— leſs.

At 36°——50 lat. and 329° long. he found the variation 7°—10', where Dr. *Halley* makes it 4°—30'.

At 45°—8' lat. and 305°—30' long. he found the variation 10°—10', which is 2°—more than in Dr. *Halley*'s chart.

From all theſe obſervations, ſuppoſing them juſt, it appears, firſt, that in the parallel of 22°—ſouth latitude, the line of no variation has ſhifted 120 leagues weſtwards, from the year 1700 to 1708.

Secondly, That the variation has increaſed on this ſide that line, while it has diminiſhed on the other; tho' in three or four places M. *Bigot* found it the ſame as is expreſſed in the chart; and in ſome others found it even greater, unleſs we rather chooſe to caſt the irregularity on the obſervations, which I believe the ſafeſt way.

The ſhip *St. Louis* went with the *Golden-fleece*, and the *Maurepas* went in 1706 for the *South-ſea*; the obſervations made on board this laſt have already been delivered by M. *Caſſini*; but the firſt quitting the other two on the coaſts of *Chili*, carried M. *Hebert* on to the *Indies*; in which voyage, beſides M. *Daumas* the pilot's journal, I have another kept by M. *Brunet*, an officer in the ſhip, who made ſeveral curious remarks, overlooked by M. *Daumas*.

This pilot putting out from *Port-Louis*, in *July* 1706, at 25 leagues north-north-eaſt of the iſland of *Porto-Santo*, near *Madera*, found the variation 5°—north-weſt, where Dr. *Halley*'s chart only makes it 4.

Cloſe by *Madeira* on the ſouth-weſt, he found it 4°——½, where Dr. *Halley* only makes it 3°——½.

Between the iſland of *Madeira*, and that of *Faro*, he found it 4, where Dr. *Halley* makes it 3.

At 50 leagues ſouth-ſouth-weſt of the iſland of *Faro*, he found the variation 3, where Dr. *Halley* makes it 2.

At 18°——15′ lat. and 357° long. he found the variation 2°—½, between which place and the bank of *Biſagos*, on the coaſt of *Guinea*, he obſerved the variation 4 times, and found it always 2°——½; Dr. *Halley* makes it in theſe places about 1°.

At 358°—long. and 6°—lat. he found the variation 2°—, as well as at 3°——15′ lat. and 10′ long. in both which places, Dr. *Halley* makes the variation about ½ a degree.

From this place, bearing to the ſouth-eaſt, as far as the equinoctial, which he cut at 7°— long. the variation in about 50 leagues courſe changed from 2 to 3°—— at 50 leagues further from 3 to 4°—— and at the end of 50 more, from 4 to 5°. In theſe places, Dr. *Halley* only makes 1°—— or 1°—½ variation, and in lieu of 50 leagues, makes 80 leagues between each degree of variation.

Having paſſed the line he bore ſouth-weſterly as far as 9°—— of ſouth lat. and 356°—15′— long. where he alſo found the variation change a degree in 50 leag. diminiſhing at this rate from 5 to 4, from 4 to 3, from 3 to 2, and from 2 to 1; ſo that at the end of about 250 leagues, there was no variation at all, and 50 leagues further, the variation was 1°—— north-eaſt; whereas hitherto it had been north-weſt; ſo that the place where the ſhip cut the line of no variation is, by their account, 100 leagues more weſtward than laid down by Dr. *Halley*.

Proceeding on towards the iſland of *Aſcenſion*, at 20 leagues north-eaſt of that iſland, M. *Daumas* found the variation 6°— ; where Dr. *Halley* alſo makes it 6°——north-eaſt.

Paſſing hence to the iſland *Grande*, on the coaſt of *Brazil*, M. *Brunet* relates, that the variation was found 11°——⅔, which is much the ſame as in Dr. *Halley*'s chart.

From hence paſſing to the ſtraits of *Magellan*, he found the variation at 12°——north-eaſt, where Dr. *Halley* makes it 12 ½, 13 where he makes it 13 ½, 16 where he makes it 16 ½, 17 where he makes 18 ½, 18 where he makes it 19, 19 where he makes 19 ½, and 19 ½ where he makes it 20°. ——All theſe obſervations I relate, becauſe they confirm one another ; with regard to which it may alſo be obſerved, that the laſt obſervation given by M. *Brunet*, was made in 40°—30′— ſouth lat. and that the ſhip having ſailed 60 leagues under the ſame parallel, he found the ſame variation in three ſucceſſive obſervations at this latitude, which perfectly agrees with the inclination of the lines of variation given by Dr. *Halley* about this place, they being inclined from eaſt to weſt, for the ſpace of 50 or 60 leagues, and thence turning inſenſibly towards the ſouthweſt in form of an *ellipſis*, as far as the ſtraits of *Magellan*.

Arriving in *December* 1706, at the latitude of 57°—10′ and 60 leagues ſouth-weſtward of the ſtraits of *Le Maire*, M. *Brunet* relates, that the variation was found 26°——north-eaſt, and that the ſame variation continued for the ſpace of 40 leagues. Here our compariſon ceaſes with Dr. *Halley*'s

Halley's chart, which does not give the variations in this ſea.

Having doubled *Cape Horn*, to the ſouth of *Terra del Fuego*, they arrived at the city *Conception*, on the coaſts of *Chili*, where M. *Brunet* found the variation 9°—30′. Hence they proceeded for *Valparaiſe*, where they found the variation 8°——. At *Piſco* and *Canete* 6°——½, and at *Callao*, the port of *Lima*, 6°——All which obſervations agree within half a degree to thoſe made on the *Maurepas*, publiſhed by M. *Caſſini*, who in examining the courſe of that veſſel obſerves, that as their latitude increaſed, the variation increaſed; to which, from theſe obſervations of Meſſ. *Brunet* and *Daumas*, we may add, that in the ſame parallel, the further they proceeded from the ſhores weſtward, the variation diminiſhed.

Thus at 44°——45′ lat. and about thirty leagues diſtance from the coaſts of *Chili*, they found 12°——variation, and in the ſame parallel at 120 leagues diſtance from the coaſt found only 7°.—

Between 40 and 41° — lat. at 10 leagues diſtance from the ſhore, they found 9°— variation, and only 6°—— at 130 leagues diſtance from the ſame ſhore.

Between 30 and 31°—— lat. and 60 leagues from the ſhore, they found 7°—— variation, and only 5 at 220 leagues.

The ſame ſhip departing from the *Conception* in *December*, 1707, doubled *Cape Horn* a ſecond time, came to anchor in the river *Gallegua*, not far from the ſtraits of *Magellan*, and from whence ſet ſail for the *Cape of Good Hope*; being, I think, the firſt ſhip that made this courſe, ſo that its obſervations are the more valuable.

At their departure from the river *Gallegua*, they found the variation 23°—north-eaſt.

At 60 leagues from this place, they found it 22°—north-eaſt.

At 30 leagues further 20°——

At 150 leagues further 18°——

At 110 leagues further 16°——

At 150 leagues further 14°——

At 60 leagues further 13°——

At 50 leagues further 12°——

At 20 leagues further 11°——

At 30 leagues beyond this 10°——

At 20 leagues further 8°——

At 100 leagues beyond this only 4°——

And, laſtly, at 120 leagues further the variation was nothing.

All theſe variations are north-eaſterly to the place where there was found no variation. — The following ones are north-weſterly.

At 60 leagues further eaſtward, they found the variation 2°——north-weſt.

At 80 leagues further 4°——

At 60 leagues beyond this 7°——

At 140 leagues further 9°—$\frac{1}{2}$.

And, laſtly, at 60 leagues further, near the *Cape of Good Hope*, where they arrived in *Mar.* 1708, the variation was found 8°——

Now, according to Dr. *Halley*'s chart, the variation ſhould have increaſed from the place of their departure in the ſpace of 240 leagues from 20 to 23°— and in the reſt of their paſſage ſhould have diminiſhed about 1° for every 2°— of longitude, as far as the line of no variation, and from hence to the *Cape*, have increaſed north-weſtward, in the ſame proportion as it had diminiſhed north-eaſtward; but it appears on the contrary, from theſe obſervations, that the greateſt variation found by them in the whole paſſage, was in this place of their departure, where it was found 23°. ——— It alſo appears, that when it did diminiſh, this diminution did not always proceed in the ſame proportion as expreſſed in Dr. *Halley*'s chart; but that during the firſt 500 leagues, the variation diminiſhed a degree for 4°——— of longitude; after which they found it diminiſhed another degree, in the ſpace of a degree and half of longitude; but henceforward the variations altered, as in Dr. *Halley*'s chart, *viz.* 1°— for 2 in longitude.

It alſo appears from theſe obſervations, that from the year 1700, the epocha of Dr. *Halley*'s chart to the year 1709, the line of no variation ſhifted 50 leagues weſtward in 35°——— of ſouthern latitude. We have already obſerved, that in the latitude of 22°—— it had ſhifted 120 leagues, according to the obſervations of M. *Bigot*, that for the 7°——ſouth, he found it 100 leagues more weſtward; and that at the latitude of 28°——north, he alſo found it more weſterly than Dr. *Halley* makes it at that latitude; ſo that the motion of this line weſtward is confirmed by many obſervations.

Proceeding from the *Cape of Good Hope* eaſtwards, they found the variation always increaſe to the diſtance of 530 leagues from the *Cape*; and in 33°—½ ſouth latitude, where they found the variation 24°—½ north-weſt, which is the greateſt they obſerved in the *Indian* ſea. Hence to the iſland *Bourbon*, and to *Pondichery* and *Merguy*, they found it leſs and leſs every day, as at their return they found it every day greater and greater to the ſame term, and about the ſame diſtance from the *Cape*. Dr. *Halley* only differs about half a degree from theſe obſervations.

The other obſervations made by theſe perſons in the *Indian* ſea, differ but little from thoſe related by M. *Caſſini*, ſo that we content ourſelves to refer to them.

Theſe are all the obſervations of the variation that the journals afforded, I ſhall ſubjoin ſome remarks made from the ſame journals for the correction, and improvement of ſea charts, eſpecially thoſe of *Pieter Goos*, which might one day be rendered much more uſeful to navigators, were a great number of ſuch journals made.

The firſt pilot of the *Royal-dauphine* took notice of the *Salvages*, which are certain dangerous iſlands to the north of the *Canaries*, whoſe ſituation therefore cannot be too exactly known by the pilots; he found them very ill laid down on the ſea charts, being placed too much eaſtward; with regard to the iſland of *Porto Santo*, at a league and half ſouth-weſt of theſe iſlands, he found the latitude 30°— Their number he obſerves is two, whereof the more northern one is the greater, from whence a chain of rocks extends for about three leagues ſouth-weſtward; at the end whereof is a little round iſland, with ſome flat ground, where the ſea beats extremely.

The firſt pilot of the *St. Louis*, in his paſſage to the iſland of *Aſcention*, found it placed by *Pieter Goos*, half a degree too much northward, its latitude he obſerved to be 20°—22′, inſtead of 19°—52′, as that author makes it.

The ſhip *Louis* we have already noted to have been the firſt which ſailed from the ſtraits of *Magellan* directly to the *Cape of Good Hope.* At its departure from the river *Galleguas*, it made eaſt-north-eaſt the ſpace of 1350 leagues, which brought it to that *Cape*; but in the way at 36°—54′ latitude, and 353°——10′ longitude, by the reckoning, they diſcovered an iſland about a league diſtance, and ſoon after two others, 3 or 4 leagues to the ſouth-weſt; and, laſtly, a 4th, to the north-weſt, which ſurprized them very much, they being 300 leagues from land.

M. *Brunet* took theſe iſlands to reſemble thoſe of *Triſtan de Cunha*, which he had ſeen in his voyage to *China* on board the *Amphitrite*; but M. *Hebert*, and the firſt pilot were of a contrary opinion, by reaſon, according to the reckoning, they had yet only made 750 leagues from the river *de Galleguas*, whereas *Pieter Goos*, placing thoſe iſlands in 12°— of longitude, makes them 1050 leagues from the ſaid river; and they were further confirmed in this opinion upon their arrival at the *Cape of Good Hope*, where by the reckoning they found 35°—— of longitude, between thoſe iſlands and the *Cape* in lieu of 26°—— according to the chart, which makes a difference of 150 leagues in this parallel. Upon this they made no difficulty of accounting them a new diſcovery, and gave them the name of the *Hebert* iſlands, or the new iſlands of *Triſtan de Cunha*.

Yet to me the opinion of M. *Brunet*, who took them for the very iſlands of *Triſtan de Cunha*

Cunha, appears much more probable. 'Tis no new thing for pilots to make new diſcoveries, and impoſe new names upon iſlands known long before them, of which we have an inſtance in the iſland of *St. Helena*, which having been placed by the firſt navigators more weſterly than it really is, was afterwards found by others more eaſterly than it had been laid down in the charts, which made them take it for a new iſland in the ſame latitude, but a different longitude from the firſt, and accordingly they gave it the name of the new iſland *St. Helena*, and inſerted it in the ſea charts; in moſt whereof, particularly thoſe of *Pieter Goos*, it is ſtill found ſo. The iſland *St. Apollonia* in the *Indian* ſea is the ſame with the iſland of *Bourbon*; and the *Iſle of Dogs* in the *South-Sea*, found by *Le Maire* in 1616, is the ſame with the iſland of *Tiburones*, diſcovered by *Magellan* in 1520; ſo in all likelihood the new iſlands diſcovered by our navigators, are no other than thoſe of *Triſtan de Cunha*. For Dr. *Halley*, who was at the iſlands of *Triſtan de Cunha* in 1700, lays them down in his chart at the ſame diſtance which the reckoning of theſe perſons required; tho' the chart of *Pieter Goos* makes them 150 leagues off; and as to the diſtance of theſe iſlands from the river *de Galeguas*, which by their reckoning was 750 leagues, 'tis true, Dr. *Halley* makes it 170 leagues more; but then the longitude, in which that author places the mouth of this river, muſt be diminiſhed by 10°——— as I have done in my map of *South America*, from ſeveral aſſured obſervations, and among others, that of an eclipſe on the 13th of *Mar.* 1653, obſerved by father *Maſcardi* in the valley of *Bucalene* in *Chili*; for by this obſervation,

ſervation, compared with others made at *Paris*, *Bucalene* is 72°—½ weſtward of *Paris*, and conſequently the river of *Galeguas*, whoſe diſtance from *Bucalene* is known, ſhould only be 68° from *Paris*; add, that by the obſervations of father *Fontaney* it appears, that the *Cape of Good Hope* is 17°——45′ more eaſtward than *Paris*; ſo that the diſtance between that *Cape* and the river of *Galeguas*, muſt be 84°——25, which comes within a degree and half of the reckoning of the *St. Louis*.

XI.

XI. *Reflections on the obſervations of the flux and reflux of the ſea, made at* Havre de Grace, *by M.* Boiſſaye du Bocage, *profeſſor of hydrography, during the years* 1701 *and* 1702, *by M.* Caſſini, *jun**. *tranſlated by Mr.* Chambers.

The place choſen by M. *Du Bocage* for his obſervations, was a part of the port beſt under ſhelter, here he erected a plank 10 feet ½ long againſt the wall of the port, and divided it into inches, whereon he obſerved the tides which happened in the day-time, having no conveniency for taking thoſe of the night.

He obſerves, that the ſea, while it roſe, drove to the ſouth-eaſt and to the north-weſt, when it fell; that the wind which traverſed the road was weſt-north-weſt, and at the entrance of the port lay in the direction of the weſt-ſouth-weſt wind.

The journal of his obſervations begins on the 9th of *April*, 1701, and ends on the 26th of *May*, 1702.

At firſt he contented himſelf with marking day by day the part of the plank to which the flood roſe, with the winds that blew both in the ebb and flow; but two months after, beginning on the 10th of *June* he obſerved the hours and minutes of high water, which he continued till the end of his obſervations, excepting a little interval between the 11th and 28th of *Nov.* during which he was obliged to have his watch mended.

Upon examining the times of high water obſerved at *Havre de Grace*, we find that on the days of full moon, the flood uſually happens a little after 9 in the morning. The earlieſt high water was on the 19th of *July*, the day of full moon, when it happened at 39′ paſt 8 in the morning; and the lateſt was on the 12th of *April*, at 39′ paſt 9; which gives a variation of an hour, in the time of the tides for the day of full moon.

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* Aug. 13, 1710.

Suppoſing the time of high water at *Havre de Grace* on the day of full moon to be at 9^h—26′ in the morning, when the time of full moon coincides with that of high water, and that an allowance is made of 2′ for each hour, that the time of full moon comes, before or after the time of high water, as already ſettled at *Dunkirk*, we we ſhall find leſs variations in the times of high water obſerved at *Havre de Grace*. For an inſtance, on the 19th of *July*, 1701, high water happened at 8^h——39′ in the morning, which is the greateſt anticipation M. *Du Bocage* obſerved. Now full moon is marked that day in the almanack at 11^h—50′ in the evening, the difference between 9^h—26′ in the morning, and 11^h—50′ in the evening, is 14^h—24′; to which at the rate of 2′ *per* hour anſwer 29′, which ſubſtracted from 9^h—26′, give 8^h——57′ for the time of high water, within 18′ of what was obſerved at *Havre de Grace*.

'Tis obſerved, that the 19th of *July*, 1701, is the ſame day on which the greateſt acceleration of high water at full moon was obſerved at *Dunkirk*.

As to the lateſt high water on the days of full moon, it was found on the 12th of *April* at 9^h——39′, the full moon happening at 0^h——13′ in the evening; ſo that here ſhould have been an anticipation of 6′, which ſubtracted from 9^h——26′, gives the time of high water on the

12th of *April*, 1701, at 9^h——$20'$ within $19'$ of what was obſerved.

Upon a like examination of the obſervations of high water made at *Havre* in the time of new moon, it appears at firſt ſight, that the floods in the new moons happen 12 or $13'$ later, than in the full moons; and yet allowing for the acceleration or retardation of $2'$ *per* hour above-mentioned, and ſuppoſing the time of high water in the new moons at *Havre*, to be at 9 —$26'$ in the morning, as in the full moons, the obſervations of the full moons will be eaſily reconciled to thoſe of the new moons, excepting two, whoſe regularity was extraordinary, there being an anticipation of ſeveral minutes in the time of high water, between one day and another, in lieu of the uſual retardation.——The new moon, wherein high water came the earlieſt, was on the 29th of *Nov.* 1701, when it was flood, at 8^h——$56'$ in the morning, and the new moon is marked in the almanack at 10^h——$11'$ in the evening of that day, the difference between 9^h——$26'$ in the morning, and 10^h——$11'$ in the evening, is 12^h——$45'$; to which anſwer $25'\frac{1}{2}$, which ſubſtracted from 9^h——$26'$, by reaſon the new moon happened on the evening, gives the time of high water at 9^h——$0'\frac{1}{2}$, within $4'$ of what was obſerved.

It is obſervable, that this new moon was the ſame as that wherein the greateſt acceleration of high water was found at *Dunkirk*, which we have attributed to the time of new moon, which happened at 10^h——$11'$ in the evening.

The new moon, wherein it was found flood the lateſt, was that of *Auguſt* the 4th, 1701, high water on that day was found at 9^h——$48'$ in the morning, and the new moon is fixed by the almanack to 10^{h}——15′ in the ſame morning. Now by the rule laid down, high water would have happened at 9^{h}——24′ in the morning, within 24′ of what was obſerved; but it is obſervable, that from the 4th to the 5th of *Aug.* there was an anticipation of 11′ in time of high water, in lieu of a retardation; ſo that nothing can be built upon this obſervation, nor even upon that of the 2d of *September*, 1701, the day of new moon, when high water was obſerved at 9^{h}——46′ in the morning, there being an anticipation here likewiſe of 20′ from the 2d to the 3d of *Sept.*

As to the higheſt tides at *Havre de Grace*, they do not uſually fall upon the day of new and full moon, but one or two days after, as has been obſerved at *Dunkirk*; and as they have likewiſe been found not only in other parts of the channel, but on the coaſts of *Africa* and *America*.

The higheſt tide obſerved at *Havre* was on the 15th of *Feb.* 1702, when the flood roſe 18 feet, 3 inch. 3 days after full moon, which had happened on the 12th of the ſame month at 3^{h}——2′ in the evening, the wind was then very ſtrong, and blew at ſouth-ſouth-weſt, which conſpiring with the tide, and driving it towards the port, might have contributed to its extraordinary elevation that day.

As to the tides near the equinoxes at new and full moons, ſome of them are higher, and others lower ſo that it can be no rule at *Havre*, that the equinoctial tides are the higheſt.

What is further remarkable is, that the heights of the tides keep exactly pace with the different diſtances of the moon from the earth in all ſtates, whether near the equinoxes, or at any diſtance therefrom, as has likewiſe been obſerved at *Dunkirk.*

kirk.——For an inſtance on the 10th of *April*, 1701, two days after the new moon ſucceeding the vernal equinox, the height of the water was found 15 feet, 6 inches, 6 lines, which is lower by 1 foot, 3 inches ½, than on the 10th of *May*; two days after the next full moon, which yet was further from the equinox, agreeably to the diſtance of the moon, which was farther from the the earth on the 8th of *April*, the day of new moon; than on the 24th of *April*, the day of full moon; ſo on the 19th of *September*, 1701, two days after the full moon next the autumnal equinox, the height of flood was found 15 feet, 11 inches, which was lower by 1 foot, 10 inches, than on the 4th of *October*; two days after next new moon, when the water was found 17 feet, 9 inches, high, one of the higheſt tides that had been known; and again on the 16th of next *October*, the day of full moon high water was obſerved 16 feet, 3 inches, which was lower than on the 4th of *October*; and on the 1ſt of *November* it was found 16 feet, 11 inches, which was higher than on the 4th of *October*.

Theſe different heights of the tides cannot be owing to the different diſtances of the ſun from the equinoxes; ſince the water was lower near the equinox, than in the other obſervations further from it; but they agree perfectly with the different diſtances of the moon from the earth, at the time of new and full moons.————For on the 17th of *September*, the moon was further from the earth than in the other obſervations on the 2d of *October*; it was nearer on the 16th of the ſame month; it was further diſtant than on the 4th, but leſs on the 17th of *September*; and on the 31ſt of *October* it was nearer than on the 17th, but further than on the 4th of *October*.

It

It would be tedious to rehearſe all the relations between the obſervations, and the diſtances of the moon from the earth ; it may ſuffice to obſerve, that on the 15th of *March*, 1702, high water was found 17 feet, 1 inch $\frac{1}{2}$, whereas on the 30th of *March*, at an equal diſtance from the equinox, it was only found 15 feet, 8 inches; and that on the 15th of *April* it was obſerved 17 feet, 6 inches $\frac{3}{4}$; and on the 28th of *April*, 15 feet, 2 inches, all which agrees with the different diſtances of the moon, which was nearer the earth on the 15th of *April*; and further off on the 28th of the ſame month, than in any other preceding obſervation, as may be ſeen in the following table:

A TABLE of the time and height of the tides,

Days and Hours of the new and full moons.					Time of high water obſerved.		Time of high water calculated.		Height of the ſea.		
	1701.	H.	M.		H.	M.	H.	M.	F.	In.	L.
●	8 Apr.	10	54	M.							
⊙	22 Apr.	5	16	E.					16	3	
●	8 May,	1	42	M.					15	2	
⊙	22 May,	2	18	M.					15	11	
●	6 June,	2	28	E.			9	16	14	11	6
⊙	20 June,	0	26	E.	9	1	9	20	15	4	
●	6 July,	0	58	M.	9	25	9	43	15	0	3
⊙	19 July,	11	50	E.	8	39	8	57	14	6	9
●	4 Aug.	10	15	M.	9	48	9	24	15	8	6
⊙	18 Aug.	2	6	E.	9	6	9	16 ½	15	4	
●	2 Sept.	6	5	E.	9	46	9	8 ½	15	11	6
⊙	17 Sept.	5	56	M.	9	30	9	34	15	4	
●	2 Oct.	2	20	M.	9	33	9	40	17	2	
⊙	16 Oct.	11	24	E.	9	12	8	58	16	3	
●	31 Oct.	11	24	M.	9	34	9	22	16	7	6
⊙	15 Nov.	5	4	E.			9	11	15	10	
●	29 Nov.	10	11	E.	8	56	9	0	17	0	
⊙	15 Dec.	10	16	M.	9	30	9	24	15	4	8
●	29 Dec.	10	47	M.	8	59	9	23	15	11	9
	1702.										
⊙	14 Jan.	1	12	M.	9	25	9	43	16	5	8
●	28 Jan.	1	57	M.	9	45	9	43	16	1	6
⊙	12 Feb.	3	2	E.	9	6	9	15	15	9	
●	26 Feb.	6	15	E.	9	25	9	8	15	6	4
⊙	14 Mar.	1	48	M.	9	34	9	41 ½	16	7	
●	28 Mar.	11	7	M	9	38 ½	9	23	15	3	
⊙	12 Apr.	0	13	E.	9	39	9	20	16	9	
●	27 Apr.	5	49	M.	9	40	9	33	15	0	9
⊙	11 May,	4	59	E.	9	23	9	11	16	3	3
●	26 May,	8	27	E.	9	25	9	4	14	1	6

in the new and full moon at Havre de Grace.

Diſtance of the ſun from the apogee of the moon.			Diſtance of the moon from the earth in conjunct. and oppoſ.	Day of the higheſt tide.	Height of the ſea.		
D.	M.	S.			F.	In.	L.
1	0	21	105589	10 April.	15	6	6
				24 April.	16	10	
				9, 10 May.	15	6	3
				23 May.	16	0	6
				8, 9 June.	15	7	
3	2	33	99713	21 June.	15	5	9
3	16	2	98372	7 July.	15	3	3
				22 July.	15	0	3
				7 Auguſt.	16	10	
				19 Auguſt.	15	11	6
				4 September.	17	6	
5	18	10	106340	19 September.	15	11	
6	1	13	93469	4 October.	17	9	
				16, 17 October.	16	3	
				1 November.	16	11	
				16 November.	15	11	
				30 November.	17	10	
				18 December.	15	6	
				30 December.	16	5	6
				1702.			
9	4	37	100013	14 January.	16	5	8
9	17	19	100477	29 January.	16	4	9
				15 February.	18	3	
				28 February.	17	2	
				15 March.	17	1	6
				30 March	15	8	
11	22	55	93519	15 April.	17	6	9
0	5	52	106496	28 April.	15	2	3
				12 May.	17	0	

If now we examine the time of high water obſerved in the quadratures, we ſhall find it happen at *Havre* 30′ paſt 2 in the evening.

Among the 23 obſervations made hereof, the flood which come the earlieſt, was on the 6th of *March*, 1702, at 1^h—55′ in the evening, and the lateſt, on the 5th of *May*, at 3^h—30′.

To frame ſome rule of this variation, we ſuppoſe the mean time of high water in the quadratures at *Havre*, to be at 2^h——40′ in the evening, and add or ſubtract from this time 2′ per hour, for the time which the quadrature noted in the almanack anticipates, or comes behind the mean time of high-water, as fixed at 2^h——40′ in the evening.

For an inſtance on the 6th of *March*, 1702, high water was obſerved at 1^h——55′ in the evening; the firſt quadrature is noted in the almanack for that day at 10^h—24′ in the evening, the difference between 2^h 40′ and 10^h—24′ in the evening, is 7^h—44′, to which, at the rate of 2′ per hour, anſwer 15^h ½, which ſubſtracted from 2^h——40′, gives 2^h—24′ ½, for the time of high water within 29′ ½ of what was obſerved.——So on the 5th of *May*, 1702, the day when the tide came lateſt in the quadratures, high water was obſerved at 3 ——30′, the firſt quadrature is fixed in the almanack at 1 ——59′, that day in the morning the difference between 1^h——59′ in the morning, and 2^h——40′ in the evening, is 12^h—41′, to which at 2 per hour, anſwer 25′, which added to 2^h——40′, give 3^h——5′, for the time of high water within 25 of what was obſerved.

The time of high water would have been found more exactly in theſe two obſervations, if in lieu of 2′ for each hour of anticipation, or retardation, we had taken $3'$, which agrees nearer with the retardation of the tides from 1 day to another, about the quadratures which uſually exceeds an hour.

High water happening at *Havre* in the new and full moons at 9^{h}—$26'$ in the morning, and in the quadratures at 2^{h}—$40'$, we have 5^{h}—$14'$ for the interval between the times of high water from the new and full moons to the quadratures.—— This interval was found at *Dunkirk* 5^{h}—$12'$, which ſhews the great conformity between the times of the tides in thoſe 2 ports.

As to the leaſt tides obſerved at *Havre de Grace*, they do not uſually happen in the 2 quadratures, but 1 or 2 days after the firſt or laſt quarter.— The loweſt tide was found on the 8th of *March*, 1702, two days after the 1ſt quadrature, the water on that day riſing only 9 feet, 8 inches, 4 lines; and the higheſt tide, as already mentioned, happened the 15th of *February*, and roſe 18 feet, 3 inches; ſo that there is a difference of 8 feet, 7 inches, between the higheſt and loweſt tides at *Havre*; whereas the difference was only found 7 feet at *Dunkirk*.

By comparing the different heights of the tides obſerved at *Havre*, about the quadratures, we likewiſe find them bear a near relation to the different diſtances of the moon from the earth, as will appear from the following table.——— Where it is obſervable, that on the 22d of *January*, 1702, the moon being in Perigeo about the quadratures, the height of the flood was found 13 feet, 2 inches, 8 lines, which is leſs only by 2 feet, than on the 28th of *April*, 1702, two days after the new moon, which was then in apogeo.

A TABLE of the time and height of the

Days and hours of the quadratures.						Time of high water obſerved.		Time of high water calculated.		Height of the ſea.		
		1701.	H.	M.		H.	M.	H.	M.	F.	In.	L.
1	□	16 Apr.	2	8	M.			3	6	11	2	6
3	□	29 Apr.	10	25	E.			2	24	12	2	6
1	□	15 May	9	6	M.			2	51	13	0	6
3	□	29 May	3	38	E.			2	38	12	3	0
1	□	13 June	2	26	E.	3	1 S.	2	40 ½	13	6	
3	□	28 June	9	14	M.	3	10	2	51	11	9	
1	□	12 July	7	17	E.	2	5	2	31	13	11	
3	□	28 July	2	23	M.	3	0	3	5	11	10	
1	□	11 Aug.	1	14	M.	2	50	3	7	13	11	6
3	□	26 Aug.	6	6	E.	2	0 ½	2	33	12	4	3
1	□	9 Sept.	9	20	M.	2	42	2	51	13	7	8
3	□	25 Sept.	7	58	M.	2	24	2	53 ½	11	3	6
1	□	8 Oct.	8	47	E.	2	29	2	28	13	6	4
3	□	24 Oct.	7	50	E.	2	30	2	30	11	7	9
1	□	7 Nov.	0	17	E.	2	51	2	45	12	5	
3	□	23 Nov.	6	0	M.			2	58	12	6	
1	□	7 Dec.	7	31	M.	2	51	2	54 ½	12	4	
3	□	22 Dec.	8	48	E.	2	39	2	28	13	6	
		1702.										
1	□	6 Jan.	5	47	M.	3	4	2	58	14	33	
3	□	20 Jan.	10	41	E.	2	13	2	24	15	3⅓	
1	□	5 Feb.	2	34	M.	2	36	3	4	12	5	3
3	□	19 Feb.	6	33	M.	2	24	2	56 ½	13	4	
1	□	6 Mar.	10	24	E.	1	55	2	24 ½	11	3	6
3	□	20 Mar.	3	47	E.	2	46	2	38	13	4	9
1	□	5 Apr.	2	9	E.	2	12	2	41	11	8	
3	□	19 Apr.	3	39	M.	3	14	3	2	12	2	9
1	□	5 May	1	59	M.	3	30	3	6	11	5	
3	□	18 May	4	4	E.	3	10 ½	2	37	12	2	8

tides in the quadratures at Havre de Grace.

Diſtance of the ſun from the apogee of the moon.	Diſtance of the moon from the earth in the quadratures.	Day of the loweſt tide.	Height of the ſea.
D. M. S.			F. In. L.
		17 April.	10 10
		1 May.	10 10 6
		16 May.	12 7 6
		30 May.	11 4
2 26 40	97615	14 June.	13 0
3 9 17	106250	29 June.	11 3
		14 July.	12 11
		29 July.	11 6
		13 Auguſt.	12 7
		28 Auguſt.	11 8 6
		10 September.	11 8 9
5 25 8	102165	26 September.	10 7
6 6 30	102275	10 October.	11 2 3
		25 October.	10 4 8
		8 November.	11 5
		23 November.	12 6
8 28 20	106425		
9 10 2	97717	22 January.	13 2 8
		6 February.	10 10
		21 February.	12 7 9
		8 March	9 8 4
		22 March.	10 10 4
		6 April.	10 6 3
11 28 56	101725	20 April.	11 6 3
0 12 40	100856	7 May.	11 6
		20, 21 May.	11 4

After aſcertaining the retardation of the tides in the new and full moons, and quadratures, we examined all the obſervations at *Havre de Grace*, and found that the mean retardation of the tide from one day to another was almoſt perfectly the ſame with that obſerved at *Dunkirk*; ſo that the ſame rule may ſerve for finding the time of high water every day in the year, in both thoſe ports. We have alſo drawn up a table of theſe retardations like that given for *Dunkirk*, by means whereof it may be found, whether the ebbing and flowing follow the ſame rule in other ports.——In order hereto, the mean time of the tide muſt be ſettled in each port, for the days of new and full moon, and quadratures for the reſt, the rules will obtain already preſcribed.

The tides in this table are marked for every 12 hours after full moon, for the conveniency of finding the morning and evening tides.

9^h 26′ M. mean time of the high tide at *Havre*, the day of the new and full moons.	2^h 40′ E. mean time of the high tide at *Havre*, the days of the the quadratures.

A TABLE of the retardation of the tides.

Days and hours after new or full moon.		Retardation of the tides.		Dif.	Days and hours after first or last quarter.		Retardation of the tides.		Dif.
D.	H.	H.	M.	M.	D.	H.	D.	H.	M.
0	0	0	0		0	0	0	0	
	12	0	26	26		12	0	32	32
1	0	0	50	24	1	0	1	8	36
	12	1	11	21		12	1	49	41
2	0	1	30	19	2	0	2	32	43
	12	1	48	18		12	3	11	39
3	0	2	6	18	3	0	3	44	33
	12	2	24	18		12	4	14	30
4	0	2	42	18	4	0	4	40	26
	12	3	1	19		12	5	4	24
5	0	3	21	20	5	0	5	28	24
	12	3	41	20		12	5	50	22
6	0	4	2	21	6	0	6	12	22
	12	4	21	19		12	6	34	22
7	0	4	39	18	7	0	6	54	20

Rule firſt, To find the time of high water at *Havre*, for the days of new and full moon, and quadratures.

Look in the almanack for the time of new and full moon, and quadratures, and take the difference between this and the mean time of high water expreſſed for the day of that phaſis; doubling this difference, you will have the number of minutes to be added to the mean time of high water, if the phaſis anticipate ſuch mean time, or be ſubtracted from the time of the phaſis, come behind the ſame, the reſult will be the true time of high water for the day of the given phaſis, whether it be new or full moon, or one of the quadratures.

For an inſtance, ſuppoſe the time of high water required for the day of new moon in *Jan.* 1702.

In the *Ephemeris* we find new moon marked on the 28th of *January* at 0^{h}—$57'$ in the morning, the difference between 0^{h}—$57'$ in the morning, and 9^{h}—$26'$ in the morning. The mean time of high water at new and full moons in this port is 8^{h}—$29'$, the double whereof, *viz.* $16'$—$58''$ is the number to be added to 9^{h}—$26'$. The mean time of high-water at *Havre* on account of the full moon's anticipating the time of high water the ſum, *viz.* 9^{h}—$43'$ is the true time of high water on the 28th of *January*, 1702. M. *du Bocage* obſerved it 9^{h}—$45'$.

Rule 2d. To find the time of high water at *Havre* for any given day.

Seek by the firſt rule the time of high water, for the day of new and full moon, or quadrature, next preceding the given day; to this add the retardation of the tide correſponding to the difference between the given day, and the day of the preceding phaſis, the ſum will be the time of high water for the day required.

For an inſtance; ſuppoſe the time of high water required for the firſt of *Feb.* 1702.

By the *Ephemeris* the phaſis immediately preceding the firſt of *February*, is new moon, which happens on the 28th of *January*, 1702, at 0^{h}—$57'$ in the morning; four days before the day given, the time of high water that day is found in the preceding example to happen at 9—43 in the morning; add to this 2^{h}—$42'$ for the retardation correſponding to 4 days, the ſum will be the time of high water on the given day, *viz.* 0ʰ—25′ in the evening. M. *du Bocage* found it that day at 0ʰ—15′ in the evening.

To find the time of high water immediately preceding, or following a time thus found, we muſt ſubtract or add the difference in the table correſponding to 12ʰ.——We may likewiſe apply to *Havre de Grace* the three laſt rules laid down for finding the higheſt and loweſt tides at *Dunkirk.*

As the interval between the new and full moons, and quadratures, there is from 6 days to 8, it follows, that when this interval is 6 days, the retardation of the tide between one day and another, muſt be greater than when the interval is 8 days.—— And hence for the greater accuracy, we have framed a table wherein the retardation of the tide is aſſigned according to thoſe different intervals.

A TABLE of the retardation of the tides.

Days and hours after the new or full moon.		Interval between the day of the new or full moons, and the days of the quadratures.								
		6 Days.			7 Days.			8 Days.		
		Retardation of the tides.								
D.	H.	H.	M.	D	H.	M.	D	H.	M.	D
0	0	0	0	29	0	0	27	0	0	25
	12	0	29	25	0	27	24	0	25	22
1	0	0	54	22	0	51	22	0	47	20
	12	1	16	21	1	13	20	1	7	18
2	0	1	37	21	1	33	20	1	25	17
	12	1	58	20	1	53	19	1	42	16
3	0	2	18	20	2	12	19	1	58	16
	12	2	38	20	2	31	19	2	14	17
4	0	2	58	22	2	50	20	2	31	19
	12	3	20	23	3	10	22	2	50	20
5	0	3	43		3	32	24	3	10	21
	12				3	56	27	3	31	22
6	0				4	23		3	53	23
	12							4	16	
7	0							4	39	

Days and hours after the quadratures.		Interval between the day of the new or full moons, and the day of the quadratures.								
		6 Days.			7 Days.			8 Days.		
		Retardation of the tides.								
D.	H.	H.	M.	D.	H.	M.	D.	H.	M.	D
0	0	0	0		0	0		0	0	
	12	0	37	37	0	31	31	0	29	29
1	0	1	16	39	1	6	35	1	2	33
	12	1	58	42	1	47	41	1	42	40
2	0	2	42	44	2	32	45	2	26	44
	12	3	26	44	3	16	44	3	11	45
3	0	4	3	37	3	50	34	3	45	34
	12	4	32	26	4	17	27	4	12	27
4	0	4	58	20	4	42	25	4	37	25
	12	5	22	24	5	5	23	4	59	22
5	0	5	46	24	5	28	23	5	22	23
	12				5	53	25	5	47	25
6	0				6	18	25	6	14	27
	12							6	41	27
7	0							7	8	27
	12									

To make uſe of this table, we find in the *Ephemeris* the numbers of days between the phaſis immediately preceding, and that ſucceding the given day, and take the retardation of the tide expreſſed in the column under that number of days.

For an inſtance; ſuppoſe the time of high water required for the 1ſt of *February*, 1702, which is the inſtance propoſed under the 2d rule.——The *Phaſis* next preceding the 1ſt of *February*, is the new moon of the 28th of *January*, and the phaſis following it, is the firſt quadrature which happens on the 5th of *February*; the interval between theſe two phaſes is 8 days: ſeek under the column

marked

marked a-top with 8 days, the retardation anſwering to 4 days, which is 2 hours 31 ; add this to 9^{h}—$43'$ (the time of high water on the day of the preceding new moon found by the rule) and the ſum will give the time of high water on the 1ſt of *February*, 1702, at 0^{h}—$14'$ in the evening within a minute of what was actually obſerved.

XII. *Reflections on the obſervations of the tides made at* Breſt, *and at* Bayonne, *by M.* Caſſini, *the ſon.* *

Having found, that the obſervations of the flux and reflux of the ſea, made at *Dunkirk*, and *Havre de Grace* agree, ſo that we may uſe the ſame rules to find in both theſe ports the time of high water, for every day in the year, preciſely enough. We have thought proper to examine if theſe rules agreed with the obſervations upon the tides made at *Breſt* and *Bayonne*, by Meſſ. *de la Hire* and *Picard*.

Theſe obſervations are related in the collection of the travels of the academy; they were made at *Breſt*, in *September* 1679, in the king's garden, which has a view to the port where the ſea is commonly very calm.

Meſſ. *de la Hire* and *Picard* obſerved there, from the 18th to the 28th of *September*, the time of high and low water. They did not wait to make their obſervations, till the tide was entirely high or low, becauſe then it remains too long in that ſtate; but they marked 2 diſtant times before and after, in which they found it at a certain exact height, which laſted ſo little, that they made no difficulty to mark even to ſeconds. They afterwards took the middle of the time which paſſed between the correſponding obſervations.

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* Aug. 16, 1710.

In comparing at firſt the retardation of the tide, from the 18th to the 19th of *September*, they found it 48′ equal to the mean motion of the moon. This retardation is afterwards a little leſs to the 26th of the ſame month; but from the 26th to the 28th, it is exceſſive, there having been from the 26th to the 27th, a retardation in the time of high water of $1^h\ 9'\ 45''$, and from the 27th to the 28th of $1^h\ 30'\ 30''$.

As the rules, that we have preſcribed to find the time of high water at *Dunkirk* and *Havre de Grace*, require the knowing the days and hours of the new and full moons, and of the quadratures, we have examined the days of the moon, upon which theſe obſervations have been made, and have found in the *Connoiſſance des Temps* of 1679, that the moon was at the full on the 20th of *Sept.* at $7^h\ 48'$ in the morning; and in the laſt quarter, the 26th, at $7^h\ 4'$ at night. The 20th of *Sept.* the day of the full moon, the height of the tides was not obſerved there: but by the compariſon of ſome obſervations that were made the preceding and following days, we ſee that it muſt have happened about 4 in the evening. We find afterwards, that the retardations of the tides was in diminiſhing, and pretty near agree with that which reſults from the rule; but from the 26th to the 28th, the daily retardation was greater, as it ought to happen according to the rule. For the laſt quarter of the moon being the 26th, it ought to have a retardation from the 26th to the 27th of 1 8′, and from the 27th to the 28th, of 1^h 24′, within a few minutes of what was obſerved from the 26th to the 27th of 1^h 10′, and from the 27th to the 28th of 1^h 30′ 30″.

For the more eaſy comparing the rule with the obſervations, we have prepared the following table; where there is marked in the firſt column, the day of the obſervation; in the ſecond, the time of the high or low water, determined by the obſervation; in the third and fourth, the time calculated according to the firſt and ſecond table; and in the fifth and ſixth, the difference between the time obſerved, and the time calculated according to the firſt and ſecond table of the retardation of the tides.

A TABLE of the tides obſerved at Breſt.

1679.	Time of the high or low-water obſerved.	Time calculated.		Diff. by the firſt table.	Diff. by the ſecond table.
		By the firſt table.	By the ſecond table.		
September.					
Days.	H. M. S.	H. M.	H. M.	M.	M.
18	2 25 30 E. H. W.				
19	3 13 30 E. H. W.				
Full moon at 7 hours 48 minutes, morn. 20		4 2 E.	4 2		
21	10 29 30 M. L. W.	10 40	10 43	10 ½	13 ½
22	11 41 45 E. L. W.	11 41	11 49	0 ¾	7 ¼
24	0 25 30 M. L. W. 0 46 30 E L. W.	0 17 0 35	0 30 0 50	8 ½ 11 ½	4 ½ 3 ½
25	1 12 30 M. L. W. 1 34 30 E. L. W.	0 53 1 13	1 11 1 34	19 ½ 21 ½	1 ½ 0 ½
1 quarter at 7 hours 4' at night. 26	1 56 40 M. L. W. 8 6 45 M. H. W.	1 53 8 6 45	1 53 8 6, 45	3 ⅔ 0	3 ⅔ 0
27	3 38 30 M. L. W. 9 16 30 M. H. W. 10 9 30 E. H. W.	2 57 9 15 9 56	2 54 9 9 9 49	41 ½ 1 ½ 13 ½	44 ½ 7 ½ 20 ½
28	10 47 0 M. H. W.	10 39	10 33	8	14

Reflections upon the obſervations of the tides made at Bayonne.

The obſervations upon the flux and reflux of the ſea, were made at *Bayonne*, by Meſſ. *de la Hire* and *Picard*, in the *Dour*, where the ſea riſes conſiderably.

They obſerved in the ſame manner, that they had done at *Breſt*, the preceding year, the time of high and low water from the 12th of *Sept.* 1680, to the 4th of *Oct.* following.

To be able to compare the rule of the retardation of the tides with the obſervations, they firſt ſought in the *Connoiſſance des Temps* of 1680, the days and hours of the *phaſes* of the moon, and they found that the moon entered the laſt quarter the 15th of *September*, 1680, at 1^h 4′ in the morning; that the new moon following happened on the 22d at 7^h 30′ in the evening; and that the firſt quarter happened on the 30th of *September*, 11^h 4 in the evening.

It follows then from the rule that to begin from the 15th of *September*, the day of the laſt quarter, there ought to have been a great retardation in the tide from one day to another, during 3 or 4 days; that from the 22d of *September* the day of the new moon to the 30th of *September*, the day of the laſt quarter, there had been an acceleration in the tides, and that from the 30th of *September*, to the 4th of *October*, there ought to have been a retardation in the tide, which entirely agrees with the obſervations.

This conformity of the rule of the retardation of the tides, with the obſervations, has given us room to examine, whether they agree in all the circumſtances. For this is ſuppoſed, that the high water happens at *Bayonne*, the day of the new and full moon at 3^h 30′, the ſame that is marked in the *Connoiſſance des Temps*. The interval between the time of the tides from the new or full moon to the quadratures, being 5^h 14, as they found at *Havre*, we ſhall have the time of high water in the quadratures, at *Bayonne*, at 8^h 44′. Upon theſe *hypotheſes* they have calculated by the rules preſcribed in the preceding memoir, the time of the high or low water in the obſervations made at *Bayonne*, ſince the 15th of *Sept.* and they have marked them in the table ſubjoined, ed, with the differences, which are for the moſt part ſo ſmall, that we could never have hoped to have been able to arrive at ſo great an exactneſs.

A table of the tides obſerved at Bayonne.

1680.	Times of high and low water obſerved.	Time calculated.		Diff. by the firſt table.	Diff. by the ſecond table.
		By the firſt table.	By the ſecond table.		
September.					
Days.	H. M. S.	H. M.	H. M.	M.	M.
12	0 1 0 M. L. W.				
	0 24 30 E. L. W.				
13	0 43 0 M. L. W.				
	1 8 45 E. L. W.				
14	1 34 30 M. L. W.				
	2 0 30 E. L. W.				
1ſt quarter,	2 35 30 M. L. W.	2 38	2 39	2	3
at 1 h. 4 m.	3 8 15 E. L. W.	3 8	3 8	0	0
M. 15	9 20 45 E. H. W	9 23	9 23	2	2
16	3 44 0 M. L. W.	3 39	3 38	5	6
	9 57 0 M. H. W.	9 55	9 54	2	3
	10 40 30 E. H. W.	10 31	10 29	9	11
18	0 13 30 E. H. W.	0 34	0 37	20	23
19	1 14 0 M. H. W.	1 7	1 13	7	1
	1 43 0 E. H. W.	1 37	1 40	6	3
20	2 7 30 M. H. W.	2 3	2 5	4	2
	2 33 0 E. H. W.	2 27	2 28	6	5
	8 42 0 E. L. W.	8 39	8 40	3	2
21	2 54 0 M. H. W.	2 51	2 53	3	1
	9 4 30 M. L. W.	9 2	9 5	2	0
	3 14 50 E. H. W.	3 13	3 18	2	3
	9 23 0 E. L. W.	9 23	9 24	0	1

A TABLE of the tides obſerved at Bayonne.

1680.	Time of the high and low water obſerved.	Time calculated.				Diff. by the firſt table.	Diff. by the ſe-cond table.
		By the firſt table.		By the ſe-cond table.			
September.							
Days.	H. M. S.	H.	M.	H.	M.	M.	M.
Full moon at 7 hours, 30 minutes, E. 22	9 39 0 M. L. W.	9	10	9	10	29	29
	9 56 30 E. L. W.	9	35	9	35	21	21
23	10 11 30 M. L. W.	10	1	9	58	11	14
	10 25 0 E. L. W.	10	22	10	19	3	6
24	10 47 0 M. L. W.	10	43	10	38	4	9
	11 2 30 E. L. W.	11	1	10	56	1	6
25	11 19 30 M. L. W.	11	19	11	12	0	7
	11 32 0 E. L. W.	11	37	11	28	5	4
26	11 50 0 M. L. W.	11	55	11	45	5	5
27	0 7 0 M. L. W.	0	13	0	2	6	5
	0 23 30 E. L. W.	0	33	0	22	9	1
28	0 35 30 M. L. W.	0	53	0	42	18	6
	0 59 30 E. L. W.	1	13	1	3	13	3
1ſt quarter, the 30th of Sept. at 11h. 4 min. E. 29	1 14 30 M. L. W.	1	31	1	26.	17	11
October.	3 19 0 M. L. W.	2	55	2	55	24	24
1	9 30 0 M. H. W.	9	11	9	8	19	22
	10 3 30 E. H. W.	9	47	9	41	16	22
2	10 52 30 M. H. W.	10	28	10	21	24	31
	11 22 0 E. H. W.	11	11	11	5	11	17
3	0 5 0 E. H. W.	11	50	11	50	15	15
4	0 27 0 M. H. W.	0	22	0	24	5	3
	1 35 30 E. H. W.	0	54	0	51	41	44

XIII. *An examination of the ſilk of ſpiders, by M. de* Reaumur [*]; *tranſlated by Mr.* Chambers.

The publick diſlike had long lain heavy on the ſpider, and notwithſtanding ſo many curious things publiſhed of it by ſeveral learned men, it was ſtill looked upon with ſome degree of horror, and held a dangerous, or at leaſt a uſeleſs inſect, till a year ago, that M. *Bon*, firſt preſident of the chamber of accounts at *Montpellier*, procured it a more favourable regard. There is room to hope, that the extraordinary things he has ſhewn of it, may be turned to ſome advantage; ſince, like the ſilk-worm, it ſpins a ſilk, capable of being made into the fineſt works; of which the gloves and ſtockings he then preſented to the royal academy of *Montpellier*, are an inconteſtible proof. A like preſent of gloves has been ſince made to the royal academy of *Paris*; where the diſcovery, having ſome air of uſefulneſs, was thought worth the purſuing; either that the publick might reap the fruit thereof, or at leaſt that the infamy might be removed of neglecting any thing that might be beneficial. The fate of the ſilk of worms is notorious, which tho' known had remained almoſt uſeleſs for many ages; and it had been inexcuſable to let the ſilk of ſpiders run the ſame fortune.

The academy therefore thought fit to depute two members, to purſue what M. *Bon* had ſo ingeniouſly hinted; and I was one of thoſe the choice fell upon. I accepted it accordingly, not only as the publick good ſeemed concerned therein, but our illuſtrious preſident, the *Abbé Bignon*, ſeemed kindly to intereſt himſelf in behalf of the poor ſpiders.

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[*] Nov. 12, 1710.

To go orderly to work, I held it my chief buſineſs to conſider the ſilk of ſpiders, with regard to that of worms, in order to find by this compariſon, whether the new ſilk would be of any ſervice, anſwerable to what we receive from the old; for it was no longer any queſtion, whether the ſpiders ſpin at certain times a ſort of ſilk fit for manufacturing; this had already been demonſtrated by M. *Bon*, paſt all exception; but whether they ſpin a ſilk that may be of advantage to the publick? To determine this, 'tis not enough to find the ſecret of feeding and bringing up ſpiders, as ſome learned men have ſuppoſed, but to find whether, granting this ſecret known, the ſpider ſilk will come as cheap as the other; or, in caſe it be dearer, whether this inconvenience be compenſated by any other advantage: Theſe two points were what I chiefly aimed at in my inquiry, and to theſe all I ſhall offer in the enſuing diſcourſe is reducible.

The artifice, which ſpiders make uſe of to catch flies, has taught all the world that they feed on thoſe inſects; but 'tis obvious, that there is no feeding a number of ſpiders, ſufficient to furniſh a ſilk manufacture with flies. What ſtratagem muſt be uſed to take the daily quantity of flies neceſſary for ſuch purpoſe? and tho' the method of catching flies was never ſo much improved, what better ſhould we be, when 'tis viſible, that all the flies in the kingdom would hardly ſupply ſpiders enough to make even an inconſiderable quantity of ſilk, as may be eaſily inferred from what we have to urge upon our ſecond article.

Recourſe therefore muſt be had to ſome new food, whereof a ſufficient quantity may commodiouſly be had. Now the rapacious diſpoſition of the ſpider is a ſufficient indication, that this is not to be had from plants, and that neither their flowers, leaves, nor fruits can afford them a proper nutriment. I did not omit, however, to try ſuch foods, leaſt I ſhould have it to reproach myſelf with neglecting any thing, and becauſe I was aware, that in matters of experiment, things frequently happen otherwiſe than was expected; but nothing I could give them of this kind proved any food at all.

Yet 'tis viſible, that flies are no [illegible] only food as may ſupply them; for tho' t[illegible]ſe and garden ſpiders live chiefly thereof, [illegible] have frequently obſerved them fall with equal [illegible]petite upon other inſects, which happen to be caught in their toils; and the ſpiders in particular, which inhabit the holes of old walls, had made me further ſenſible, that no inſect comes amiſs to them; for, upon viſiting their cells, I have uſually found the carcaſſes of diverſe kinds of inſects therein, as millepedes, caterpillars, butterflies, *&c.*

My buſineſs therefore ſeemed to be to find ſome inſect, whereof there is ſtore enough to be eaſily had, and nothing but earth-worms appeared to anſwer this view; as to quantity, the gardens and fields are full of them; witneſs the allies of gardens, which, after a rainy night, are covered over with little round bits of earth, each whereof covers a hole, at which an earth worm had aroſe, nor is there any difficulty in taking them, provided you go in the night with a candle, and remember never to ſeek for them after a fit of dry weather.

'Tis true, I have never found any earth-worms in ſpiders webs, or holes, but this was no great objection, conſidering the weight of the inſect,

and

and its keeping altogether on the ground, which makes it impoſſible for the worm ever to come within the ſpider's clutches. In effect, my expectation was anſwered; for ſhutting up ſeveral large ſpiders of diverſe kinds, which had overlived the winter, and given them pieces of worms to feed on, they were preſerved alive thereby.

But it was by no means enough to convince me, that this food was proper for ſpiders, to find them live ſeveral months without any other; an experiment, I had formerly made, rendered this very precarious, having kept a houſe ſpider alive above 3 months, without any food at all. 'Tis known beſides, that the young ſpiders, hatched in *September*, lived 8 or 9 months without eating.

But as I had incloſed my ſpiders in boxes, covered with glaſs, I could eaſily obſerve whether they meddled with the food that was given them, and accordingly have frequently ſeen them attack pieces of worms, which would ſtir, notwithſtanding their ſeparation from the reſt of the body, after the like manner as we find them attack inſects, which have ſome ſtrength ſtill left, after being taken priſoners in their nets, the various motions of the pieces of worms having ſerved to ſtir up theſe inſects of prey. It muſt be added, that they maintained their bulk and vivacity, which thoſe I had left without food did not; and what is ſtill more deciſive, ſeveral of them made balls or cods, and laid eggs therein.

I afterwards tried other kinds of foods, to ſee whether they might not be equally proper for them; for how commodious ſoever worms might be, fleſh would have been ſtill more ſo: but I found, that they did not affect it, and that if it fell in their way, they rarely faſtened on it. The reaſon may be, that the fierce and rapacious diſpoſition of the ſpider, muſt be urged and excited by living animals.

Yet I bethought myſelf of another food, which might make up this defect, by the exquiſiteneſs of its taſte, the young ſpiders, which have juſt left their cods, preferring it to all others; but what determined me to uſe it, was the reſemblance it bore to the tender fleſh of inſects, which the ſpiders delight in, I mean the ſubſtance, which fills the feathers of young birds, before they be arrived at their full growth. 'Tis known, that upon plucking ſuch feathers, they are bloody at the end, and that their quill or barrel is ſoft; ſo that either ſqueezing, or cutting it up, we find it repleat with a ſoft ſubſtance, interſperſed with numerous veſſels, which diſcharge blood upon cutting them; having plucked ſuch feathers from young pidgeons, and even old ones, which had ſometime ago been ſtripped of the large feathers of their tail and wings, I divided them into little pieces, a line, or ½ a line long, and thus threw them to the ſpiders, who ſeemed well pleaſed therewith, and eſpecially the young ones; ſo that I could ſee 5 or 6 cluſters of them about a ſingle piece, each of them ſucking at the ſide it was cut on.

Thus far things ſucceeded to my wiſh, proper foods being found out, which alone ſeemed wanting; and 'tis likely other equally commodious ones might be diſcovered, even among the inſect-kind, tho' what we have already propoſed is eaſier to come at than the leaves of mulberries, wherewith ſilk worms are fed. It may be had without any trouble in all countries, and without running any danger from ſharp winters. The poulterers will furniſh young feathers enough, or they may be had by breeding up chickens, or pidgeons, and pulling off their feathers from time to time, which will not hinder their laying eggs, and bringing young ones, as I have found by experience; but it will appear from what follows, that a large deduction is to be made, when we come to raiſe ſpiders enough to furniſh a ſilk manufacture.

As ſoon as the young ſpiders quit the ſilken ball they were incloſed in, they appear perfectly at peace, and work in concert upon the ſame web, ſome of them ſpreading new threads over thoſe already drawn by others; but ſuch union does not hold long. I had diſtributed 4 or 5 thouſand ſpiders, juſt freſh from their balls, into ſeveral boxes, putting 2 or 3 hundred in ſome, and one hundred, or fifty, even leſs in others. The boxes were about the length and breadth of a card, and were as high, and as wide, which was room enough for ſuch little animals. Finding them gather upon the glaſs, which covered the boxes, I made an aperture in each about a line below the glaſs, and thruſt a card through the ſame, which reſted on the width of the box, and ſtopped the aperture cloſe enough to prevent the ſpiders eſcaping. On this card, I laid ſuch food as I had found ſuitable for them, diſpoſing it ſo near the glaſs, or upper ſurface of the box, that the ſpiders might have their food at hand; and to make thoſe at the bottom, or on the ſides of the box, go in queſt of it, I had taken care to punch ſtore of holes in the card; by this means a great number of ſpiders might be fed in a very little time; for the firſt days one might ſee them fall on their food with eagerneſs, ſeveral of them faſtening upon the ſame bit of feather.

But their natural fierceneſs ſoon declared itſelf; the biggeſt and ſtrongeſt took a liking to eat the ſmaller and weaker ſort. In effect, every time I viewed them, I could ſee ſome little one, which had fallen a prey to another ſomewhat bigger; ſo that in a ſhort time there was ſcarce one or two left in a box.

I knew that the large ſpiders ſometimes fight together, when they chance to meet, but was of opinion that they would grow more ſociable by being bred together, as we find the chickens and turkies, raiſed in the ſame yard, live amicably enough, tho' they always make war upon new comers. But the truth is, theſe young ſpiders devour each other much more than the big ones: whether it be that theſe laſt ſtand leſs in need of food, or that being heavier, and more bulky, they do not care to ſtir.

This inclination of the ſpiders to eat one another, is, in ſome meaſure, the cauſe that their number is ſo ſmall, conſidering the prodigious quantity of eggs they lay. 'Tis true, there are ſeveral ſorts of inſects that prey upon them. *Pliny* mentions ſome ſpecies of hornets and lizards, which make them their ordinary food. I have ſeen the little brown wall lizard catch them, with infinite addreſs; but notwithſtanding all this, we ſhould find them incomparably more numerous were it not for their eating one another.

No other way therefore ſeems left for bringing up ſpiders, but to lodge them ſeparately. One may provide, for inſtance, boxes, which are ſubdivided into ſeveral little apartments, forming ſo many diſtinct cells, which I have practiſed accordingly; but to give each ſpider its food apart, would occaſion an expence very diſproportionate to the profit accruing from them. This, however, might be bore withal, were not the ſilk of worms to be had on better terms.

I am ſenſible, that ways might be found of abridging this trouble of giving them food, and even have contrived ſome myſelf, which I do not think neceſſary to deſcribe here; but do what you will, 'tis ſtill to be feared, that much more time will be employed therein than is neceſſary to give ſilk-worms their food.

The diſtributing the ſpiders into ſeparate cells, does likewiſe draw on a new difficulty, which greatly leſſens the advantage they have over ſilk-worms in point of fecundity; for the benefit of this cannot be had without keeping a large number of eggs, which have been fecundified by copulation, nor is this practicable without putting ſpiders together. I am ſenſible there is a time when a milder fermentation of juices diveſts theſe animals of their natural fierceneſs, and that they may then be put together without danger; but how ſhall we know this preciſe time, which withal is to be a little before that when they are diſpoſed to lay eggs? It would be eaſy finding, whether they lay all their eggs nearly on the ſame days of the year; but there are ſeveral months difference between the times when ſome lay from that of others.

The fecundity of ſpiders is prodigious, as is fully ſet forth by M. *Bon*; but, after all, the like may be ſaid of ſilk-worms, even tho' we ſhould only ſuppoſe them to lay about a hundred eggs, whereof hardly 40 afford worms that make their balls; whereas ſpiders lay 6 or 7 hundred, and yet all the ſilk-worms I have brought up on this occaſion, yielded at leaſt 3 or 4 hundred eggs a-piece. 'Tis obvious therefore that the number of ſilk-worms might be multiplied, as much as we pleaſed, did that only depend on the quantity of their eggs, as abundantly appears from the large ſtock of ſilk they now produce in *Europe*, where anciently there were no worms at all; it would be eaſy therefore in courſe of time to have a quantity of ſilk-worms, as much ſurpaſſing thoſe we now have, as theſe ſurpaſs that little number firſt brought from the eaſt; but what hinders us from increaſing our ſtock is the trouble of lodging, feeding, and attending them; for if the quantity of ſilk was increaſed, its price would be diminiſhed, and, in that caſe, would not pay the charges of raiſing the worms. Silk-worms therefore ſeem clearly to have the advantage over ſpiders, as to the facility of raiſing them; ſo that we are to have no great expectations from the new ſilk, unleſs it have ſome other advantage over the old, either in reſpect of beauty, ſtrength, or the quantity it yields, which makes our ſecond article of inquiry.

As all the ſorts of ſpiders do not afford a ſilk capable of being manufactured, and as thoſe which do furniſh ſuch ſilk, only ſpin it to form balls, or cods, wherein to lay the eggs; for as to the webs they make to catch inſects, they are uſually too fine for any purpoſes of ours, it ſeems neceſſary to give a general idea of ſeveral ſpecies of ſpiders, to which all the reſt are reducible, and of the different manner wherein their ſeveral balls are formed, in order to ſhew which of them afford ſilk to the beſt advantage in theſe countries.

M. *Bon*, who has likewiſe conſidered ſpiders, with regard to their ſilk, reduces them to two principal kinds, *viz.* the long-legged ſpiders and the ſhort-legged ones, which laſt, he informs us, alone furniſhed the new ſilk; but this diviſion, which would have great advantages, on account of its ſimplicity, ſcarce ſeems ſufficient to diſtinguiſh the ſilk ſpiders from others: for how ſhall we know preciſely which to call ſhort-legged ſpiders, and which long-legged ones? There are ſpiders, whoſe legs are of a middle ſize, between the longeſt and ſhorteſt, and on the which of the two kinds are theſe to be ranged? On the footing of the former diviſion, it would be difficult to decide whether they yield a ſilk or not; but this is not the worſt, for the diviſion would be apt to lead people into uſeleſs pains, the generality of ſpiders, which, according to this, ſhould promiſe moſt ſilk, yielding none at all. Such are the ſeveral kinds of wandering ſpiders, and the large brown ſpiders inhabiting the holes of old walls, whoſe legs are ſhorter than moſt of thoſe which yield ſilk, though they yield none themſelves.

I rather chooſe to range all ſpiders under two claſſes; the firſt including the ſeveral ſpecies, comprized by M. *Homberg*, under the general name of wandering ſpiders, being ſuch as do not lay nets for catching of inſects, but roam abroad in queſt of them. Theſe ſpiders ſpin but little, and this only to make a cod, or cover, for their eggs, which ſome of them make hemiſpherical, and leave it hung upon ſtones, or hid under ground, or even upon trees, or among herbs; others give it the figure of a ball, and are too fond ever to ſtir from it, but carry it about with them, always hanging to the *papillæ* about their *anus*, in ſuch manner that the ball only ſeems to make one body with the ſpider; which, upon this occaſion, appears a little bigger than it ſhould do. Catching one of them, and taking its ball away, you will ſee the ſpider reſume it, as ſoon as ſet at liberty, with great eagerneſs, taking it in its feet, and clapping it firſt under its belly, and here letting us into the artifice it uſes in its ordinary carriage; for bending its hind part till it reach the little ball, and then rubbing the ſame ball very ſwiftly with the *papillæ*, a viſcid liquor is preſt out of the ſame, and ſmeared over part of the ball, which by this means is ſtuck faſt to the *papillæ*. The parts thus rubbed are eaſily diſtinguiſhed, as being whiter, and cloſer than the reſt; nor does the animal's tenderneſs end here, but when its young ones are hatched, it bears them on its back, where the young ſpiders likewiſe diſcover a notable dexterity in their arrangement about their mother's body, being ſuch, that there is no perceiving them when ſhe walks, only her body, upon this occaſion, appears ſomewhat rougher than uſual.

The texture of the balls of this kind of ſpiders, is very cloſe, and uſually white or greyiſh; but, beſide that it only yields very little ſilk, what it does yield, is not fit for manufacturing.

The ſecond claſs includes all the ſpiders, which lay nets for catching inſects. Theſe I divide into four principal kinds, each whereof might be ſubdivided into ſeveral ſpecies, if an exact hiſtory of ſpiders were intended: the firſt kind contains all the ſpiders which make webs of a cloſe texture, and ſtretch them as parallelly to the horizon, as the weight of the web will admit. Such are the houſe ſpiders, which lay their webs in the corners of rooms, and ſome ſpecies of garden ſpiders, which make like webs, and placed after the like manner as thoſe of houſe ſpiders.

Theſe all of them incloſe their eggs, which ſtick a little to one another, in a ſort of web, not unlike either as to ſtrength or colour. The common ones they lay for flies; ſo that we have but little to expect from theſe towards a ſilk manufacture.

The ſecond kind includes the ſpiders, which inhabit the holes of old walls. Theſe line the wall all around their hole with a web, and within the hole itſelf, make another web of a tubular figure, by which they enter and come out of their holes; but neither is the ball, wherein theſe ſpiders incloſe their eggs, any ſtronger than their common web; ſo that theſe two are out of the queſtion.

In the third kind I rank all the ſpiders, whoſe nets have no proper texture reſembling that of a web, but conſiſt only of a number of threads ſtretched every way. This kind might be ſubdivided into a great number of ſpecies, which form their balls in different manners; ſome make them ſegments of a ſphere, and faſten the flat ſide to a leaf, and the firmneſs wherewith they brood over them is invincible; for notwithſtanding their natural fierceneſs, if the leaf be taken away this ball is faſtened on, the mother ſpider will go with it, and never leave it, unleſs by force, till the young ones are hatched. Theſe balls are very white, and of a cloſe texture; others make two or three little ruddy balls, and lay their eggs therein, hanging them up by threads in ſome open place, but taking care to cover them with dry leaves, to prevent paſſengers from ſeeing them; to this purpoſe hanging their leaves upon thread at ſome diſtance from the ball; others make their ball of a pear form, and hang it by a thread, like the pear ſuſpended by its pedicle.

Theſe balls are all of a cloſe texture, but their ſilk too weak for working; perhaps, thoſe in the pear form might be of ſome uſe; but they are ſo ſmall, and conſequently contain ſo little ſilk, as ſcarce to be worth minding.

Laſtly, the fourth kind includes thoſe ſpiders, which make webs conſiſting of ſeveral threads, all placed in the ſame plane, and proceeding from the ſame point, like ſo many *radii* of a circle, which terminate in its circumference. Acroſs all theſe threads, goes another thread, which winding ſpirally, is faſtened in diverſe places upon each thereof. Theſe webs are uſually perpendicular to the horizon. M. *Homberg* calls this ſort the garden ſpider; and accordingly we find it very common among wood, buſhes, *&c.* Under it are included ſeveral ſpecies of ſpiders, different in ſize, colour and ſhape.

Theſe ſpiders lay their eggs one upon another, in ſuch manner that the maſs they compoſe, has the figure of a flatted ſphere, or rather an elliptic ſpheroid. Some of them glue their eggs to one another, by a viſcous matter wherewith they are moiſtened, when they come from the body; but thoſe of others are left looſe. The inmoſt threads, which inveſt theſe eggs, are wound cloſer than the reſt, which are but looſly twiſted, much like the outward threads of the balls of ſilk worms.

Moſt of theſe ſpecies of ſpiders ſpin a ſilk, which is fit for manufacturing; tho' what ſome of them may afford is too weak to ſuſtain a common loom.

Spider ſilks may be had of more diverſity of colours than that of worms, which is always white or yellowiſh; whereas the ſpiders balls, beſides white and yellow, afford ſky blue, brown, and a fine coffee colour.

The ſpiders, which yield the coffee-coloured ſilk, are very ſcarce, at leaſt I have met with none, but in a few broom fields, where I likewiſe found ſome of their balls, the ſilk whereof is very ſtrong and beautiful. They are formed after a different manner from all the ſpider balls above-mentioned; the eggs are incloſed in a brown ſilk looſly wound over them as in other balls; but this brown ſilk is itſelf incloſed in another ball of greyiſh ſilk, whoſe texture is very thick and cloſe, and much like what remains of a ſilk worm ball, when part of it has been wound off.

The ſpiders lay eggs, and ſpin ſilk to incloſe them in ſeveral months of the year, not only in *Auguſt* and *September*, as M. *Bon* has obſerved; but ſome of them likewiſe in *May*, and others in the following months, thoſe which lay ſo early are ſuch as have ſurvived the winter; whereas thoſe M. *Bon* ſpeaks of, are only hatched in the ſpring, and conſequently lay their eggs much later than the former.

Spiders, we have already ſhewn, ſpin two kinds of threads; one ſerving for a web wherewith to catch inſects; and the other only as a cod, or caſe, to keep their eggs in; but it may be proper here to add, that the two threads only differ from each other by their greater or leſs ſtrength: and to explain how the ſpider is able to ſpin a thread of this or that degree of ſtrength at pleaſure, we ſuppoſe it already known, that near the *anus* of the ſpider are ſeveral *papillæ*, which do the office of a wier-drawer's iron, and mould or faſhioned the liquor, which paſſing through them, and afterwards drying, becomes the ſilk. The ſpiders, we have here to do withal, *viz.* thoſe which afford a ſilk proper for manufacturing, have 6 ſuch *papillæ*, 4 of them very ſenſible, and two ſcarce viſible without a magnifier, being placed near the baſes of the two large *papillæ* neareſt the *anus*. Each of the

6 viſible ones is itſelf compoſed of other leſs and inviſible ones, as may eaſily be ſhewn, by ſqueezing the belly of a ſpider between two fingers, till the liquor in the *papillæ* begin to iſſue forth, and then applying another finger upon one of them, for drawing it gently away again, ſeveral threads will follow it, all of them viſibly diſtinct from each other, from their very riſe, and which of conſequence muſt have paſt thro' different holes. Theſe threads are too fine to be well told; yet this I can affirm, that I have frequently ſeen above 8 or 10 ſpring from a ſingle *papilla*. In effect, more or fewer of ſuch threads are drawn from a *papilla*, according as the finger is preſſed ſtronger, or upon a larger part of its end; whence it is eaſy to conceive how the ſpider makes its thread finer or ſtronger at diſcretion; ſince not only the applying more of the 6 viſible *papillæ* againſt a ſolid body, but even applying them more forcibly, or a larger part of each produces a thread, conſiſting of a greater number of other threads, and conſequently bigger and ſtronger.

There muſt be about 18 times as many threads, ſuch as they iſſue from the holes of the *papillæ*, to make one of the threads of a ball, as a thread of a web. If the quantity of threads, which go to the one and the other, be proportional to the ſtrength; for a weight of 3 grains, hung to one of the threads of a web, uſually breaks it, whereas 40 grains will not break a thread of their ball.

But if theſe threads of the ſpider's ball be ſtronger than thoſe of its web, they are weaker than thoſe of the ſilk-worm's ball, tho' in a leſs proportion. A thread wound from one of theſe laſt balls will uſually ſuſtain 2 drams and ½; and conſequently a thread of a ſpider's ball is, to that of a ſilk worm's ball, about as 1 to 5, which ſeems to be another advantage of the ancient ſilk above the new.

Indeed each thread of a ſpider's ball is ſlenderer than that of a ſilk-worm's ball, nearly in the ſame proportion as it is weaker; but this hardly makes up for the diſadvantage, ſince the more threads, the more difficulty there is in joining them; to ſay nothing of the danger, that all the threads do not draw alike, and conſequently that their aſſemblage have not the ſum of the ſtrengths each thread would have a part. This multiplicity of leſſer threads, which compoſe each thread of a ſpider's ſilk, to make it as thick as that of a ſilk-worm, may likewiſe contribute to render ſuch works as are made of this ſilk leſs gloſſy than thoſe of common ſilk; for what we call gloſs or luſtre in a ſtuff, being only the effect of its reflecting more coloured light of any kind than another ſtuff of the ſame colour, the more little interſtices there is in a thread of ſilk, the leſs gloſſy it muſt appear, ſince it will reflect the leſs light. Now 'tis obvious, that the little interſtices will be more numerous in a thread, thus compoſed of ſeveral different ones, than in another thread of the ſame thickneſs, wherein there is no diſtinction; the parts of the viſcid liquor they are formed of, touching each other in more places than ſeveral diſtinct threads can poſſibly do. Suppoſing therefore each thread of ſpider's ſilk to be of itſelf equally gloſſy with that of a ſilk worm, 'tis certain, that when 5 ſuch threads are combined together, to make another of that natural thickneſs, of a ſilk-worm's thread, this compound thread, and the work formed thereof, will be leſs gloſſy than thoſe of the common ſilk-worm.

This would be true on a ſuppoſition that each ſimple ſpider's thread is naturally as gloſſy as a ſimple ſilken thread; but this ſuppoſition is, perhaps, too favourable to the ſpider's ſilk, for we find, that the more frizled a thread is, the leſs gloſſy it is found: thus we ſee, that wool, whoſe fibres are naturally more curly than thoſe of ſilk, is leſs gloſſy withal. Now each hair or thread of ſpider's ſilk is certainly more frizled, than a hair of the worm's ſilk, and conſequently muſt have leſs luſtre. The reaſon of this diverſity is evidently owing to the manner of winding, or reeling the two; for in winding a thread looſely, the ſprings of all its little parts are left at liberty to act with all their ſtrength; and thus to bend or curl it this way and that according to their ſeveral directions; whereas, in winding a thread after a cloſer manner, the action of the ſpring of the component parts is prevented, and even weakened by the violence of the new ſituation. This will be more readily allowed if we can conſider, that the firſt threads of a ſilk-worm's ball, which themſelves are twiſted in, in a looſer manner round the reſt, are much leſs beautiful and gloſſy than thoſe which form the body of the ball, which are wound very cloſely.

This ſame looſe manner wherein the ſpider's threads are wound, contributes on another account to diminiſh the luſtre of the ſilk they furniſh, *viz.* by preventing their being wound, or reeled, as thoſe of ſilk-worms are; and obliging the balls to be carded before they be ſpun: hence it is obvious, that the large threads of ſilk when they come from the ſpinner, muſt conſiſt of an infinite number of ſhort hairs, and conſequently cannot appear with the beauty, evenneſs, and luſtre of another thread of the ſame thickneſs, compoſed only of hairs, equal in length to itſelf, by reaſon all the ends of ſuch ſhort hairs muſt needs produce little inequalities in the compaſs of the thread, which will weaken its luſtre. If any proof of this were needed, it were eaſy to obſerve, that the ſilk even of the ſilk-worm balls, when carded, is much inferior to that reeled directly from the pods.

Suppoſing that only two of the *papillæ* had furniſhed threads to make a ſpider's web, and that each of thoſe *papillæ*, which uſually furniſh threads compounded of ſeveral others, had only furniſhed a ſimple one, the threads of the web being 18 times weaker than thoſe of the ball; this laſt thread, which we have already obſerved is 5 times ſmaller than that of a ſilk-worm, muſt conſiſt of 36 hairs or threads at leaſt; this reflection may, perhaps, aſſiſt the imagination to conceive the prodigious diviſibility of matter; for, how minute muſt a thread be viſible, notwithſtanding, to the eye, which is no bigger than the hundred and eightieth part of a ſingle hair of ſilk, which hair itſelf is only a two hundredth part of a thread of the fineſt ſewing ſilk? For I have frequently divided ſuch threads into two hundred leſſer ones, ſo that a thread of ſpider's ſilk of the thickneſs of common ſewing ſilk, muſt really conſiſt of 36000 threads, and may be actually divided into 1000.

A thread of ſpider's ſilk, compoſed of theſe 36000 ſimple threads, may, perhaps, be ſomewhat bigger than a thread of common ſilk, compoſed of 200 ſimple threads, tho' the ſum of the bulks of the 36000 threads, and the 200 be the ſame, by reaſon it would be difficult, or perhaps impoſſible, to arrange ſuch a number of threads together, without leaving ſeveral vacuities between them, by which the bulk muſt be increaſed; it is for this reaſon that the ſpider's ſilk has been thought to yield more in the working, than the common ſort; but if it had been conſidered, that in return for this, it muſt be weaker in the ſame proportion, inſtead of conſidering it as an advantage, people would rather have looked on it as a defect, ſince a large body of this ſilk will only have the ſtrength of a leſs body of the other.

We are now come to the laſt point, *viz.* to conſider what relation the quantity of ſilk each ſpider will furniſh yearly, bears to that procured from a ſilk worm. By carefully weighing ſeveral ſilk-worm balls, I find, that the larger kind weigh 4 grains, and the leſſer 3, ſo that ſuppoſing 16 ounces to the pound, at leaſt 2304 worms will be required to produce a pound of ſilk. A perſon cloathed in ſilk little imagines, that ſeveral thouſand worms have wrought all their life-time to furniſh the matter thereof.

I have weighed with equal care, a great number of ſpider balls, and have always found, that about 4 of the larger ſort go to balance one ſilk-worm ball, and that they weigh about a grain a-piece; ſo that 4 of the largeſt ſpiders would be required to yield as much ſilk as a ſingle worm affords, ſuppoſing there were no more waſte in one kind of ſilk than the other; but in reality the ſpider balls are liable to a conſiderable diminution, from which thoſe of the ſilk-worms are exempted. This diminution ariſes hence, that the ſpider balls when weighed, are full of the ſhells of eggs, from which the young ones have been hatched, together with other kinds of filth; ſo that calculating the diminution of theſe balls, we muſt deduct above two thirds from their weight, ſince from 13 ounces of foul ſpider ſilk, M. *Bon* only procured 4 ounces of pure ſilk; whereas ſilk-worm balls undergo no diminution, or at leaſt ſo little, that it is compenſated by only putting that of ſpider ſilk at two thirds of the whole.

It has been already noted, that a ſpider ball before it be cleanſed, is to the weight of a ſilk-worm ball, as 1 to 4; and thererefore when cleanſed, its weight will be to that of the other, as 1 to 12; 12 therefore of the largeſt ſpiders will only furniſh as much ſilk as one worm; but each worm makes it ball, as being neceſſary for the metamorphoſis it has to undergo; whereas it is otherwiſe with ſpiders; for their balls being only made to lay their eggs in. If with the whole current of naturaliſts before M. *Bon*, we ſuppoſe their *ſpecies* to conſiſt of males and females, and not as he does, take them for hermophradites, only the female ſpiders muſt make balls; whence it follows, that ſuppoſing the number of males and females nearly equal, 24 of the largeſt ſpiders will only afford as much ſilk as one worm.

No leſs than 55296 of the largeſt ſpiders therefore will be neceſſary to make a pound of ſilk, which ſpiders muſt have been fed ſeparately for ſeveral months. In this view, their ſilk which ſeems to coſt us much more than it is worth, being at leaſt 24 times as expenſive as that procured from worms; and this ſuppoſing it were not neceſſary to keep the ſpiders a-part; but that each ſpider would only take up the room of a worm, which however is a falſe ſuppoſition, for there muſt room enough be allowed each to make its web; if one were to purſue the calculus, therefore, of what this ſilk would coſt, ſuppoſing the animals fed ſeparately, and a competent apartment allowed each, it would evidently appear, that the ſpider's ſilk, would coſt incomparably more than that of the ſilk-worm.

Let it be added, that what has been hitherto ſaid, only holds of the largeſt ſpiders; for if the eſtimate were made in the common garden ſpiders of this country, twelve of them would hardly be found to yield as much ſilk as one of the balls above-mentioned; 280 of them would only afford the quantity of ſilk contained in one ball of a ſilk worm, and conſequently 663,552 ſpiders hardly one pound of ſilk.

'Twill be thought pity, that ſuch little hope ſhould be left of benefitting by ſo ingenious a diſcovery; but there is ſome proſpect of reſource; ſpiders may poſſibly be found, which ſhall afford more ſilk, than thoſe commonly ſeen in this country. 'Tis confirmed to us, by the accounts of all travellers, that thoſe of *America* are much larger than ours; whence it ſhould likewiſe follow, that their webs and balls are proportionably larger. The ſilk-worms, which the natives of far diſtant countries have multiplied ſo much in *Europe*, may give us hopes, that the *American* ſpiders may likewiſe be propagated here, at leaſt 'tis worth making the experiment, which alone ſhould decide in a thing of this nature; for my own part, nothing that any where relates to the preſent ſubject ſhall be omitted by me; tho' if any thing uſeful be ſtruck out, the chief glory will be M. *Bon*'s due.

XIV. *Experiments on the effect of the wind with regard to the thermometer, by M,* Caſſini *the ſon* *.

Among ſeveral phyſical obſervations, that M. *l'Abbé Teinturier*, the arch-deacon of *Verdun*, has ſent me ſince his return from *Paris*; he has obſerved that when we raiſe a wind with bellows againſt a thermometer, the liquor that is incloſed therein increaſes its height; which ſeems contrary to the impreſſion, that the wind makes upon us, which is a ſenſation of cold.

To examine, if it had the ſame effect upon our thermometers, I applied a pair of common bellows to a thermometer ſhut up in a chamber, which in the caves of the obſervatory, ſtands at the height of 50 degrees, and was then at 52 degrees; that is, two degrees above temperate; and after having blowed for 7 or 8 minutes againſt the ball, the thermometer roſe 1 degree.

I repeated the ſame experiment ſome days after, the thermometer was at 46 degrees, and it roſe alſo a degree in the ſame time.

I made uſe of one of M. *Amontons* thermometers, which I had applied to the fire-place of a forge, where there had not been any fire for many years. This thermometer roſe almoſt a line in the ſpace of 6 minutes that I blowed againſt it.

I at laſt put the ſame thermometer to the fire-place of the forge, where I left it for three hours or thereabouts. I afterwards took it away to ſee at what height it was, which I marked 53 inches, 2 lines $\frac{2}{3}$. I blowed againſt it for 5 minutes, and taking it away, I found it at the height of 53 inches,

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* Dec. 13, 1710.

ches, 4 lines $\frac{1}{3}$; that is, 1 line $\frac{2}{3}$ higher. I put it in its place again directly, and after having blowed for 5 minutes, I found it at 53 inches, 5 lines $\frac{1}{4}$, Having in the laſt place blowed for 5 minutes more, it roſe to the height of 53 inches, 5 lines $\frac{5}{6}$; ſo that in the ſpace of $\frac{1}{4}$ of an hour, the thermometer roſe above 3 lines.

We may offer for a reaſon of this experiment, that all motion produces heat, and that thus the air being excited with violence, acquires ſome degree of heat; ſo in effect, it ſeems to cauſe in us a ſenſation of cold, becauſe the particles of air being driven with violence, apply themſelves with more force, and in a greater quantity againſt our bodies, which are warmer than the air that we breathe.

XV. *Experiments on the thermometer, by M.* de la Hire *the ſon**.

My father had formerly obſerved, that having covered with ſnow the ball of a ſpirit of wine thermometer expoſed to the air, but not to the wind, the ſpirit of wine did not alter its height in the tube, and that afterwards having blown with a pair of bellows ſtrongly againſt this ſnow, the ſpirit of wine had remained at the ſame height; from which it ſeems, that we might conclude that the temperature of the air, which acts upon the ſpirit of wine, could not cauſe any alteration therein, being ſtrongly driven againſt it; nevertheleſs it has appeared quite the contrary, by an experiment related to the academy, by M. *Caſſini* the ſon. It is to endeavour to diſcover the reaſon of this contrary effect, that we have again made the experiment that he has related, but in different circumſtances, and upon 4 thermometers; 3 of which were ſpirit of wine, and the 4th an air, one of M. *Amonton* 's.

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* Dec. 17, 1710.

Nov. 27, 1710, about 11 in the morning, we blowed with a pair of bellows ſtrongly againſt the ball of a ſpirit of wine thermometer, expoſed for a great many years in the eaſt tower of the obſervatory, which is uncovered in ſuch a manner, that it is ſheltered from the wind, and the ſpirit of wine, that was at 35 parts in the tube, which marks an air a little warmer than the beginning of froſt, having obſerved, that when it is at 32, it begins to freeze in the country, did did not riſe ſenſibly in the tube; we had taken the precaution before we uſed the bellows to put them for 2 hours in the ſame place where the thermometer was, for fear that if the whole bellows were a little warmer than the air which enters in, it might grow warm in them, and afterwards meeting the ball of the thermometer might heat it, and make the liquor riſe; and on the contrary, if the bellows had been in a place where the air was colder than that where the thermometer was, it might make it fall, as we obſerved in blowing with the ſame bellows; and pretty ſoon after the above experiment, againſt the ball of another thermometer, which was in my father's cloſet, where the air was much warmer than the outward air, where theſe bellows had been expoſed; for the liquor preſently ſunk about ½ a line, and afterwards roſe again almoſt to the ſame height, altho' we continued blowing.

We have alſo made another experiment upon an air thermometer, which was one of thoſe that M. *Amontons* firſt made for the experiment of the heat of boiling water. The ball, which is at the bottom of the little bent tube, is very big, and has in its lower part quickſilver enough to furniſh the dilatation of the air of the ball, which makes it riſe into the tube that is open at the top, and is about 4 feet high, ſo that the air does not enter at all into the tube.

Nov. 27, 1710, at 4 in the afternoon, the thermometer and the bellows having been in the ſame place above 5 hours, and having marked exactly the height of the quickſilver in the little tube, we blowed for 3′ againſt the ball filled with air, which was compreſſed by 25 inches of quickſilver, and we did not obſerve any alteration of height in the quickſilver that was in the tube.

The next day, at 10 in the morning, we repeated the firſt experiment upon the thermometer, which is in the eaſt tower, and the ſpirit of wine did not riſe ſenſibly; there was another ſpirit of wine thermometer near that, the ball of which was much ſmaller, and the tube very thin, which we took away and put in a cloſe place juſt by, where there was a pair of double bellows; and after having left it there 3 or 4 hours, we blowed againſt this ſecond thermometer for 7′, with the double bellows, and the ſpirit of wine roſe 3 lines in the tube.

We afterwards took M. *Amonton's* air thermometer, which had been a great while in this place, and blowed againſt the ball, with the double bellows for 7′ and the quickſilver alſo roſe 3 lines; indeed there were 3 or 4 of us a little diſtant from the thermometer, during the experiment.

We were afraid, that the number of perſons that there were had cauſed this effect; for this reaſon we left the thermometers near one another for 2 or 3 hours, and afterwards with a common pair of bellows, we blowed for 3′ againſt each of the balls of both theſe thermometers, the ſpirit of wine and the quickſilver which were fallen again to the heights that they were before the preceding experiment, both roſe again about a line, but that of the ſpirit of wine rather leſs than the other.

We were afraid it was becauſe we had begun with that of ſpirit of wine, and that the bellows were heated in our hands; for this reaſon, we left them in the ſame poſition, and the bellows near them, and at 6 at night we blowed again for 3′ againſt both the balls, beginning with the ſpirit of wine thermometer, which roſe a little; but the air one did not riſe at all.

We afterwards blowed with the ſame bellows againſt the ball of a ſpirit of wine thermometer of M. *Amontons*, which is placed in my father's cloſet, where the air was warmer than that where the bellows were, and the ſpirit of wine roſe in the tube $\frac{2}{3}$ of a line, and did not fall at firſt, as it had done the day before.

The 4th, at 7 in the morning, the air thermometer and the great ſpirit of wine thermometer, and the bellows having been all night in the eaſt tower, we blowed for 4′ againſt the ball of the air one, and it did not riſe at all; we then blowed againſt the ball of that of ſpirit of wine, and it roſe about a line. Afterwards we blowed for above 4′ againſt the ball of another ſpirit of wine thermometer, which is ſmaller, that we had left near the windows of a place juſt by, which is cloſe and expoſed to the ſouth, the holes of the bellows being turned againſt the windows, the liquor hardly roſe at all; but by continuing to blow, with the holes of the bellows turned the other way, it roſe more.

At 2 in the afternoon, the ſame thermometer having remained in the ſame place, and received the impreſſion of the ſun for 3 hours and $\frac{1}{2}$, and the bellows being laid in the place upon a ſeat about 6 feet from the thermometer, the ſun having alſo ſhined upon them, we blowed againſt the ball of this thermometer, the liquor fell more than 6 lines, the holes of the bellows not being turned againſt the windows, but upon turning them againſt the windows, and continuing to blow, the ſpirit of wine fell again conſiderably, tho' the weather was very hot, the ſky having been very clear all the day, and the ſun ſhining there during the experiment.

The 5th, in the morning, we carried the air thermometer and the little ſpirit of wine one into the cave of the obſervatory, and after having left them there almoſt $\frac{3}{4}$ of an hour, and the bellows alſo, and having opened and ſhut them for ſome time, that they might take in the ſame warmth as that of the air of the cave, we blowed for $5'$ againſt the ball of the air thermometer, and the quickſilver roſe about 3 lines: but as both the thermometers were a foot diſtant from one another, and as before, we blowed againſt that of air, we had alſo obſerved the height of that of ſpirit of wine, we perceived that the ſpirit of wine thermometer was alſo riſen 1 line, altho' it had not been at all blowed againſt; we afterwards blowed for the ſame time againſt that of ſpirit of wine, and it alſo roſe about 3 lines, during which time the air thermometer did not riſe at all.

We had taken the precaution to carry them into the cave, fearing leaſt the light diffuſed in the air in the day, might make ſome impreſſion upon it, which had ſome relation to what happened to the *Bolonian* ſtone and other *phoſphorus*.

Afterwards we applied a piece of cloth two or three times double againſt the ball of the air thermometer, and blowing violently againſt it, it roſe only one line, and during this time the ſpirit of wine thermometer, which remained at the ſame place, roſe ½ a line; we afterwards applied the cloth againſt the ſpirit of wine thermometer, and having blowed againſt it for the ſame time, it roſe alſo ½ a line: but the air thermometer did not riſe at all during this time, any more than in the preceding experiment.

Altho' in general, the experiments which we have juſt related, ſeem to deſtroy the old one, that my father had made, they nevertheleſs furniſh a means of giving the reaſon of explaining the difference found between them.

For the ſnow, which was upon the ball of the thermometer, and thro' which the air being driven by the bellows paſſed, was cold enough to chill the particles of air, a little leſs cold than the ſnow, which were applied in a large quantity, and in a little time, by the means of the bellows againſt the ball of the thermometer, and had made the liquor riſe. One can hardly doubt, but that this is the true reaſon of this laſt experiment, and it ſeems, by the means of it, we may give the reaſon for all the differences that we have obſerved in thoſe that we have made. But before we abſolutely decide, we believe it is neceſſary to wait, till we have made the two following experiments: the firſt would be to blow againſt the ball of a thermometer, during a very great cold; and the ſecond, during a very great heat, that we might ſee, if what ſhould happen in the extreme, was conformable to that which happened in and about the mean ſtate.

The 16th, at 8 in the morning, a ſpirit of wine thermometer, and ſome water in a veſſel, being put all night in the ſame place, we put this thermometer into the water, and after having left them there a good while, we did not at all obſerve that the ſpirit of wine had changed its height in the tube; we afterwards drew the thermometer out of the water, we wetted a cloth in this water, we applied it two or three times double upon the ball of this thermometer, and we blowed ſtrongly with a common pair of bellows againſt the linnen for 4 or 5′, without the ſpirit of wine changing its height.

Having left the thermometer in this ſtate for an hour, we had a mind to repeat the experiment. We took away the linnen from the ball of the thermometer to make the ſpirit of wine take the ſame degree of heat, as the air of the place where it was; and in waiting till it had taken it, we had a mind to ſee, if, by agitating it in the air, there would not happen the ſame thing to it as by blowing upon it, which ſucceeded; for having ſhaken it ſtrongly in the air for 8′, the ſpirit of wine roſe 2 lines in the tube; afterwards having let it reſt ſome time, it did not change the height at all. We afterwards put it for 8′ into the ſame water where it had been at firſt, and the liquor fell about a line, but this was only during the 4 laſt minutes; we drew it out of the water and having applied the wet cloth to it, we blowed ſtrongly for 8′ againſt the cloth, and the ſpirit of wine roſe again to the ſame height that it was at before it had been plunged in the water.

The 17th, at 9 in the morning, the ſame ſpirit of wine thermometer having remained all night in the eaſt tower of the obſervatory, and ſeveral pieces of marble which we had put there, we applied them to the ball of this thermometer, and in half an hour the ſpirit of wine ſunk in the tube above a line, and afterwards continuing to examine it, we perceived that it was a little riſen. During this experiment, the great ſpirit of wine thermometer, which is always in this tower, was riſen about 2 lines $\frac{1}{2}$. This experiment would ſeem to prove, that the marble cooled more than the ſpirit of wine.

XVI. *Obſervations on the little eggs of hens without yolk, vulgarly called cock's-eggs, by M.* Lapeyronie, *of the academy of* Montpelier; *tranſlated by Mr.* Chambers.

The prejudices of birth and education frequently feed people up in errors, even upon matters of fact, which it is no leſs incumbent on academies to diſabuſe them of, than to tell them new truths.

'Tis a common opinion, even among perſons of ſenſe, that cocks lay eggs, and that theſe eggs being hatched in a dunghill, produce ſerpents with wings, called baſiliſks[*]; and what is more, they hold, that the very looks of theſe baziliſks ſtrike people dead; yet the whole fable has no other ground than an ancient tradition, whoſe falſity will be demonſtrated by the following facts.

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[*] *Sunt etiam quædam ova majora, alia minora, alia etiam minima quæ vulgo in Italia* Centinina *dicuntur & mulieres noſtræ hodie (ut olim) a Gallo edita & baſiliſcus productura fabulantur. Vulgus (inquit Fabricius) putat exiguum hoc ovum eſſe ultimum Gallinarum, cum jam centum ova gallina pepererit (unde* Centininum *vocant) quod ſine vitello eſt: habet tamen cætera, ut chalazas, albumen, membranas, & corticem, veriſimile enim eſt tum generari, cum vitelli omnes jam in ova migrarunt, neque amplius in vitellario aliquis ſupereſt vitellus, qui in ovum evadere poſſit: ex altera tamen parte; albaminis adhuc modicum ſupereſt: ex hoc enim modico credibile eſt ovulum propoſitum creari.* Harvæus in tractatu generationis animalium, exercitatione xii. de ovorum differentiis.

A farmer brought me ſeveral eggs, one of them bigger than a pidgeon's egg †, aſſuring me they had been lain by a young cock, which was the only one in his yard, where however there were ſeveral hens; of this, he made ſo little queſtion, that he poſitively engaged, that in caſe I would hatch any one of theſe eggs, a ſerpent ſhould ariſe from it; and to convince me of this, he told me, I need only open one of them, which I ſhould find without any yolk, and that in the room of yolk, there would be the viſible figure of a ſerpent.

I opened one of theſe eggs in the preſence of M. *Bon*, and ſeveral other perſons, who were all equally ſurprized to find it quite void of yolk, and in lieu of yolk, to diſcern a body much reſembling a little twiſted ſerpent *; which I eaſily unfolded, after firſt ſtiffning its ſubſtance in ſpirit of wine, when it appeared as in *fig.* 3. After this, I opened ſome others, which, in the main, proved all like the former, with this only difference, that the ſeeming ſerpent was not equally well expreſſed in all. Several of theſe eggs I ſhewed the academy; in ſome of which was ſeen a yellow round ſpot about a line in diameter, and without thickneſs, ſituate on the membrane next under the ſhell, directly againſt the obtuſe end of the egg.

The difference between theſe and the common eggs, which have all a yolk, gave me a curioſity to examine into the matter, being convinced, that if they were lain by a cock, it muſt have had a peculiar organ for the purpoſe; and beſides the teſticles and two yards have been furniſhed with an ovary and a tube, which muſt have rendered it an hermaphrodite. This indeed is no more than ſeveral animals are in their kind; and we read ſo many accounts of ſuch monſters, that there was no great difficulty in ſuppoſing, that a cock might chance to be ſuch.

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† Plate I. Fig. 1.

* Fig. 2.

Upon this thought, I opened the young cock, which was ſuppoſed to have lain the little eggs, and in the diſſection I made of it, found two large teſticles, from whence aroſe very regular ſeed veſſels, which terminated each in their ſide, with a little yard in the *cloaca*. The cock in fine was found very vigorous, but utterly incapable of laying for want of organs.

Upon this, I procured ſome of the eggs to be brooded on for hatching; but opening them after a month's incubation, I could find no alteration in them, excepting that the white was more divided, and alſo more fluid than uſual.

The farmer having now no cock was ſurprized to find a continuation of the ſame eggs, and being very ſolicitous to find whence they ſhould come, watched them ſo narrowly, that at length he aſſured me, they were lain by a hen, which he brought me accordingly.

All the time I kept this hen I found it crow very ſtrongly, much like a cock that was hoarſe: and that ſhe voided by the *anus* a thin yellowiſh matter, not unlike the yolk of an egg dilated in water, and ſhe ſtill continued laying little eggs like thoſe I had opened.

Being ſatisfied of theſe facts, nothing remained but to diſcover the cauſe, which accordingly I ſought for in the *viſcera* of the hen, and ſhewed the academy a bladder about the bigneſs of the fiſt full of limpid water repreſented by CCCCC, *fig.* 4. which was faſtened by the upper root of its neck G, to the ligament EE, whereby the ovary is fixed to the mouth of the oviduct, and by the lower root to the centre G of the meſentery of the oviduct, which choaked as it were the two parts of the oviduct thus graſped by F F.

This ſort of dropſy pinched the two parts F F of the oviduct ſo cloſe, that their cavity, even when moſt diſtended, was not above five lines in diameter; and conſequently a common egg, ſuch as they are when they fall into the tube, could not paſs without burſting either the oviduct or itſelf.

The belly was full of a yellow liquor, wherein little concretions were found floating like pieces of hardened yolk, which formed another very ſingular ſpecies of dropſy.

Now the large bladder full of water was the real cauſe of all theſe effects.——When an egg graſped by the mouth of the tube became ſeparated from the ovary, and entered into the oviduct, it paſſed along, tho' with ſome difficulty, beyond the firſt choak, but could not get beyond the ſecond, both as it was cloſer than the firſt, and by reaſon the white of the egg had been enlarged by an acceſſion of moiſture, furniſhed from the membranes of the tube it had paſſed by: thus the egg being impriſoned between the two choaks, irritated the membranes of the tube, which not being able to expel it, redoubled its contractions, and obliged the hen to give itſelf violent throws and ſtruggles, which it expreſſed by a ſort of cry, imitating, as already has been obſerved, the crowing of a cock. By theſe ſtruggles the bladder of water was compreſſed, and thruſt cloſe againſt its hold; the effect of all which forces was, that the egg, whoſe membrane was yet very thin, and which have but very little white, and no ſhell, burſt; upon which the yellow run out ſometimes into the abdomen, and ſometimes into the anus, according to the ſituation of the chink, or rupture; and both the one and the other had befallen this hen, as we have already obſerved.

The bulk of the egg being thus diminiſhed by a loſs of a great part of the yolk, came forth notwithſtanding the choak, and was at length delivered.

It is obſervable, that the white ſurrounding the yolk, filled notwithſtanding its being pierced in the part whereat the yolk run out; and conſequently its wanting the tenſion, one would have thought neceſſary for its growth; for notwithſtanding all this, the humour furniſhed by the membranes of the * oviduct, continued to fill and ſwell its ſpungeous parts; and in proportion as it ſwelled, it expreſſed the remainder of the fluid matter of the yolk, which could not withſtand by reaſon of its iſſue; and therefore commonly was evacuated almoſt wholly, only leaving ſometimes a few traces in one of the corners of the egg, under the form of a yellow ſpot. It might ſometimes alſo happen, that a little part of the yolk ſhould be left in a body, though I have never found it ſo in any eggs I have opened.

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* Several perſons pretend, that the white of the egg is furniſhed by the yolk. This obſervation demonſtrates, not only that the yolk is not the ſource of the white (for how could the yolk, which increaſes rather than diminiſhes in the oviduct, be ſufficient to produce the whole ſubſtance of the white, which is of a much greater dimenſion than the yolk itſelf, if it did not receive it elſewhere?) but alſo that the liquor which makes it, does not paſs through the yolk, but after having paſſed through the exterior membrane of the egg, it enters immediately into the ſpongious body, where it ſtops; if this was otherwiſe, the humour of the white would have flowed together with the yolk, and its ſponge would not have increaſed.

While the yolk was emptying, in itſelf, by little and little, the *Chalazas* ranged themſelves differently, according to the place where the egg had burſt; if the rupture were aſide of a *Chalaza*, the cells about the oppoſite *chalaza* ſwelling, choſe the other which faſtened itſelf to the obtuſe end of the egg, as meeting there with the leaſt reſiſtance; accordingly I have frequently found it faſtened in this part, and ſometimes even together with the yellow ſpot.

But when the aperture happened in a part of the yolk equally diſtant from the two *chalazas*, they then wrought in concert to expel the yolk; and after this, reunited in the centres of the egg, by the contraction of the membrane of the yolk, to the ends whereof they are ſtrongly faſtened, which made the reſemblance of a ſerpent much more twiſted than when there was only one *chalaza*.

After the yolk had been intirely evacuated, and even followed by the moſt fluid part of the white, its aperture was ſoon cloſed up, and cicatrized by the viſcidity of the white, as well as by the fatty matter ſmeered over the inſide of the oviduct, and even by the humid matter of the egg-ſhell lodged at the bottom of this duct.

I have collected ſome of this humour, and expoſing it to a gentle heat, it formed a ſubſtance perfectly like the ſhell.

There is a great probability, that part of the white went away with the yolk, ſince there was only about a third ſo much white in one of theſe, as in a common egg.

I have ſometimes found the *cicatrix* of the aperture of the membrane whereat the yolk had iſſued ſo cloſely faſtened to the part of the ſhell correſponding to it, that one could not have ſeparated it without tearing, which was not the caſe in the reſt of the circumference.

If hens ſometimes lay eggs without ſhells, it is owing either to ſome diſeaſe, which irritating the tube, makes them expel the egg before its time; or too great a fecundity, which will not give them leiſure to ripen them all; ſome hens will, in the ſame day, lay a perfect orderly egg, and another without a ſhell; add, that the want of a ſufficient quantity of this humour which forms the ſhells, may in ſome hens be a cauſe thereof. It is poſſible likewiſe for a hen to lay eggs like thoſe here deſcribed, when by the violence of its ſtruggles to be delivered of it, or by ſome other external cauſe the yolk of the egg happens to be burſt in the oviduct; but this cauſe not being conſtant, the ſame hens may at other times lay well conditioned eggs.

Such choakings or compreſſions which utterly prevent or annihilate the young ones in the oviparous kind, by withdrawing the matter of the nutriment, would only render thoſe of the viviparous kind monſters, by reaſon theſe do not carry their nutriment with them, but find it in the womb where they are going, provided ſuch compreſſion do not deſtroy any part eſſential to the life of the animal.——Hence we are not to be ſurpriſed, that theſe latter furniſh much more monſters than the former.

An explanation of the figures, tranſlated by J. M.

Plate I. *fig.* 1. repreſents an egg without yolk, of the natural bigneſs, covered with itsſhell, having one angle acute, and the other obtuſe.

Fig. 2. repreſents the ſame egg opened.

AAA the clear white.

BB the thick white, in the centre of which appears the *chalaza*, or figure of the pretended ſerpent.

Fig. 3. repreſents the ſame *chalaza* taken from the centre of the thick white, and viewed through a microſcope.

Fig. 4. ſhews the inſide of the belly of the hen, which laid the eggs repreſented in the preceding figures.

AAAA the ovary.

BBBBBBBB the oviduct.

CCCCC the preternatural bladder filled with clear water, ſituated in the middle of the *abdomen*, laid on one ſide to diſcover its faſtenings; it covers a part of the oviduct, and ſtrangles it in the 2 places marked FF.

D the expanſion, or entrance of the oviduct.

EE the ligament, which faſtens the ſide of the right expanſion of the ovary.

FF the ligament of the neck of the bladder CCCCC, which ſtrangles two places of the oviduct.

GG the faſtening of the ligament; the upper one to the ligament EE, and the lower one to the centre of the meſentery of the oviduct.

H the *cloaca* in which are ſeen two apertures; one of which correſponds with the oviduct, and the other with the inteſtine.

IIIII ſeveral yellow concretions, interſperſed in the *abdomen*, reſembling parcels of the yolk of a hard egg.

XVII.

XVII. *A compariſon of the obſervations of the eclipſe of the moon,* Feb. 13, 1710, *made in different places, by M.* Maraldi*.

We have compared the obſervations of this eclipſe made at *Montpellier*, by Meſſ. *Plantade* and *de Clapiez*, with thoſe which are made at the ſame time at *Verſailles* and at *Paris*; and this is the reſult of them.

h	′	″	
11	15	50	All *Grimaldi* out of the ſhadow at *Montpellier*.
11	9	0	*Grimaldi* intirely emerged at *Verſailles*.
	6	50	Difference of the meridians between *Verſailles* and *Montpellier*; but *Verſailles* is more weſterly than *Paris* by 50″ of time; therefore the difference of the meridians between *Paris* and *Montpellier*, is 6′ 0″.
11	23	53	The eclipſe was 8 digits at *Montpellier*.
	19	0	The ſame phaſe at *Paris*.
	4	51	Difference of the meridians.
11	32	40	The eclipſe is 7 digits at *Montpellier*.
	28	0	It is 7 digits at *Paris*.
	4	40	Difference of the meridians.
11	29	0	*Copernicus* out of the ſhadow at *Montpellier*.
11	22	0	The ſhadow quits *Copernicus* at *Verſailles*.
	7	0	Difference of the meridians between *Montpellier* and *Verſailles*, and between *Montpellier* and *Paris*, 6 10″.

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* March 12, 1710.

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Fig.

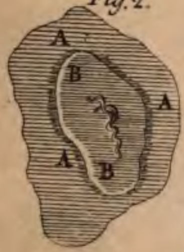

Fig. 2.

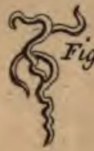

Fig. 3.

J. Mynde sc.

h	′	″	
11	42	0	The ſhadow at the edge of *Menalaus*, at *Montpellier*.
11	36	15	The ſhadow at *Menalaus*.
	5	45	Difference of the meridians between *Montpellier* and *Paris*.
11	47	2	*Pliny* at *Montpellier*.
10	40	0	*Pliny* begins to emerge at *Verſailles*.
	7	2	Difference of the meridians between *Verſailles* and *Montpellier*, and between *Paris* and *Montpellier* 6′ 12″ by this obſervation.
11	47	2	*Pliny* at *Montpellier*.
11	41	20	The ſhadow of *Pliny* obſerved at *Paris*.
	5	42	Difference of the meridians.
11	53	18	All *Tycho* out of the ſhadow at *Montpellier*.
11	47	10	The ſhadow at the ſecond border of *Tycho* at *Paris*.
	6	8	Difference of the meridians.
12	12	24	The eclipſe is one digit at *Montpellier*.
12	8	0	The ſame phaſe at *Paris*.
	4	24	Difference of the meridians.
12	18	10	End of the eclipſe at *Montpellier*.
12	12	20	End of the eclipſe at *Paris*.
	5	50	Difference of the meridians between *Paris* and *Montpellier*.
12	11	30	End of the eclipſe at *Verſailles*
	6	40	Difference of the meridians between *Verſailles* and *Montpellier*.

By the compariſon of theſe obſervations, the ſpots of *Grimaldi*, *Copernicus*, and *Tycho*, give within ſome ſeconds the ſame difference of the meridians between *Verſailles*, *Paris*, and *Montpellier*; and that of 6′ 10″ which reſults between *Montpellier* and *Paris*, agrees with that which has been found by the obſervations of the ſatellites of *Jupiter*, made in both places, and by the triangles of the meridian. The difference of the meridians which reſults between *Paris* and *Verſailles*, by the ſame obſervations, is alſo the ſame that has been found between theſe 2 cities, by geometrical operations.

This exactneſs comes from the ſpots being well determined, and the time of the ſhadow's quitting the ſecond limb of theſe ſpots being well diſtinguiſhed. There is not the ſame exactneſs in the difference of the meridians which reſults from the ſpots of *Pliny* and *Menelaus*, obſerved at *Paris* and *Montpellier*; becauſe as theſe ſpots have ſome breadth, they have not agreed perhaps in marking the arrival of the ſhadow at the ſame term of the ſpot, as they have done in the obſervation of the former ſpots.

The difference of the meridians which reſults from the determination of the digits eclipſed, is a little more than that of the ſpots; which ſhews, that we muſt not make uſe of theſe ſorts of obſervations in the finding of longitudes, except when we have no obſervations of the ſpots.

Laſtly we ſee, that in the determination of the end of this eclipſe, all the obſervers agree within about ½ a minute, tho' they are not ſo exactly agreed in the determination of the digits eclipſed.

A
TABLE
OF THE
PAPERS contained in the Abridgment of the History and Memoirs of the Royal Academy of Sciences at Paris, for the Year MDCCXI.

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I. *On the communication of air in water.*

II. *On the cauſe of the variation of the barometer.*

III. *The deſcription of a natural grotto near* Foligno *in* Italy.

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AN

ABRIDGMENT

OF THE

PHILOSOPHICAL DISCOVERIES and OBSERVATIONS in the HISTORY of the ROYAL ACADEMY of SCIENCES at *Paris*, for the Year 1711.

I. *On the communication of air in water.*

WE know that the water is quite filled and impregnated with air. As ſoon as it is *in vacuo*, the air which it contained, diſengages itſelf, and goes out, in an infinite number of bubbles. The mechaniſm of the reſpiration of fiſhes, conſiſts only in drawing from the water, the air that is incloſed in it; but Meſs. *de la Hire* had a mind to ſee what power obliges it to enter therein, and whether it enters with a velocity proportioned to the force with which this power impells it.

For this purpoſe, they took a crooked glaſs tube, with unequal branches, the longeſt of which being hermetically ſealed, was twenty-four inches, and the ſhorteſt three. They poured water in it, inclining it, but did not fill it intirely; ſo that when they afterwards placed it vertically, there happened to it the ſame thing, as to a tube not quite filled with quickſilver. There was at the top of the long branch, ſome air a little dilated; it occupied four inches, and the water kept up at ſixteen inches, nine lines, above the water of the little branch: theſe four inches of air, and theſe ſixteen inches, nine lines, of water, made there-

M 2

fore an *equilibrum*, with the intire column of air which preſſed upon the little branch; and as they had taken the time when the barometer was in its mean height, this column was equivalent to 27 $\frac{1}{2}$ inches of quick-ſilver, or thirty-two feet of water, which are three hundred eighty-four inches: conſequently the four inches of air incloſed in the long branch, made an *equilibrium* with three hundred ſixty-ſeven inches, three lines of water, and were more dilated than the outward air in the ratio of three hundred eighty-four, to above three hundred ſixty-ſeven.

The air which touched the water of the little branch, being more condenſed, or which comes to the ſame thing, more preſſed than that of the long branch, ought therefore to enter into the water, to paſs into the long branch, to riſe always through the water, to join itſelf to the air of the top of the tube, to augment its bulk, and its weight, and make the ſixteen inches, nine lines, of water, ſink. To make the outward air enter into the water in a greater quantity, the little branch opened into a glaſs phial, which preſented to the air, a pretty large ſurface.

This was done the 16th of *March*, 1710; and the crooked tube was left in experiment. Meſſ. *de la Hire* fully expected, that the water of the long branch would ſink, as they had ſeen it happen to that of a water-barometer which they had; they believed alſo, that beſides its deſcending in general, by the introduction of a new air into the top of the tube, it would have ſome particular variations of height, by the ſame cauſes with the barometer and thermometer; but the event was abſolutely contrary to all that could be foreſeen. At the end of three months, the water was riſen about four lines in the tube; and the 26th of *December* it was an entire inch; ſo that the air which was incloſed in it had loſt $\frac{1}{4}$ of its bulk. The variations of the heat, and of the gravity of the atmoſphere, had not any effect upon this water.

Meſſ. *de la Hire* do not yet undertake to explain ſo unexpected, and ſo odd a phænomenon. They labour to clear it by other experiments, which perhaps have alſo their oddneſſes, or their wonders.

II. *On the cauſe of the variation of the barometer; tranſlated by Mr.* Chambers.

It is evident from the barometer, that when it rains, or is about to rain, the air uſually becomes lighter than before. Now 'tis eaſy to imagine, that if the air become lighter, it muſt rain; for that the little inſenſible molecules of water diffuſed in vaſt quantities through all parts of the air, being no longer ſufficiently ſuſtained, when the air has loſt part of its weight, muſt of courſe begin to fall; and by this fall, united together, form drops of rain: thus in the receiver of an air-pump, after about half of the air is exhauſted, and conſequently the weight of the whole diminiſhed by half, we find a little ſhower trickle down. ——— But then, how comes the air to looſe its weight? The anſwer might be, that where it rains, the winds have driven part of it elſewhere; but M. *Leibnitz*, in a letter to the *Abbé Bignon*, alledges another reaſon, entirely new.

He aſſerts, that a foreign body immerged in a fluid, weighs with that fluid, and make part of its whole weight, ſo long as it is ſuſtained therein; but that if it ceaſe to be ſuſtained, and conſequently begin to fall, its weight will no longer make part of that of the fluid, which therefore becomes lighter of courſe. ——— The application of this, to the molecules of water, is eaſy; they increaſe the weight of the air while it ſuſtains them, and diminiſh it when it lets them fall; and as it may frequently happen, that the higher mocules of water ſhall fall a conſiderable time ere they join the lower, the weight of the air will be diminiſhed ere it rain, and the barometer will foretel it.

This new principle of M. *Leibnitz* is very ſurpriſing; for a foreign body immerged in a fluid, whether it be ſuſtained therein, or not, muſt ſtill weigh,or gravitate. And can its weight bear on any other bottom, than that which ſupports the whole fluid? Does this bottom ceaſe to bear the foreign body, becauſe it falls? And does not this body, even in its fall, continue to make part of the fluid ſo far as it regards its weight? By this account, while a chymical precipitation is proceeding, the whole matter muſt weigh leſs than it did, before the precipitation began, which is more than was ever yet obſerved.

But notwithſtanding all theſe objections, the principle will ſubſiſt when cloſely examined. What ſupports a heavy body is preſſed by it; a table, for inſtance, which ſupports a lump of iron of a pound weight, will be preſſed by it; and will only be preſſed, becauſe it ſuſtains the whole action and effort, which the cauſe of weight, whatever it be, exerts on this lump of iron to drive it downwards; if therefore the table yield, and give way to the action of this cauſe of weight, it will no longer be preſſed by it; and conſequently will not bear any thing: after the ſame manner, the bottom of a veſſel containing a fluid, oppoſes the whole action of the cauſes of gravity on that fluid; and if a foreign body float therein, the bottom will alſo oppoſe the action of the cauſe of gravity thereon; for that ſuch body being *in equilibrio* with the fluid, is as to this reſpect, a real part of it: thus the bottom is preſſed, both by the fluid, and the foreign body; and it bears them both; but if ſuch body fall, it yields to the action of gravity; and conſequently the bottom does not ſuſtain it, nor will ſuſtain it till the body falls upon it, during the time of the fall; therefore the bottom is eaſed of the weight of this body, which is no longer bore by any thing, but impelled by the cauſe of gravity, which nothing hinders it from yielding to.

To confirm this doctrine, M. *Leinbitz* propoſed an experiment.———To the two ends of a thread faſten two bodies; one of them heavier, and the other lighter than water; but ſuch as that, both together, may float on the water: put theſe in a phial full of water, and ſuſpend this phial in a ballance, where it ſtands exactly in equilibrio with another weight; then cut the thread which ties the two bodies together, and the heavier will begin to fall. In this caſe he holds, that the phial will no longer maintain the equilibrium; but that the weight, which before was equal to it, will now prevail, and make it mount by reaſon the bottom of the phial, which bears the whole weight thereof, is now leſs preſſed than before. It is evident ſuch phial muſt be of a ſufficient length, that the falling body may not arrive at the bottom, before the phial have had time to mount. In chymical precipitations the veſſels are not deep enough, or the matters precipitate too haſtily, or ſometimes even too ſlowly, for the effect of this principle to appear, ſo that the falling particles always, as to ſenſe, in equi-librio, with the liquor which contains them.

M. *Ramazzini*, a celebrated profeſſor at *Padua*, to whom M. *Leibnitz* had propoſed his experiment, made it with ſucceſs after a few fruitleſs tryals, and it ſucceeded in the ſame manner with M. *de Reaumur*, to whom the academy deputed the care of making it, ſo that we have here a new view in philoſophy, which tho' it ariſe from a principle well known, is yet very ſubtile and abſtruſe; and may give us room to apprehend, that in ſubjects which had been the moſt canvaſſed, many things have ſtill eſcaped us.

III. *The deſcription of a natural grotto near* Foligno, *in* Italy.

M. *Maraldi* has given the deſcription of a natural grotto, which has been found in making the foundation of a houſe, that M. *le Marquis Eliſei* built three miles from *Foligno*, in *Italy*. The figure of the grotto is irregular; its greateſt height, which is unequal, is thirty or forty feet, and ten or twelve paces broad. The walls of it are formed by a fine incruſtation of marble, of a pale yellow colour; and they are raiſed from ſpace to ſpace, by columns in *baſſo relievo*, of the ſame marble. From the top of the vault there deſcended other like columns, ſome quite to the ground, which were twenty-five feet long; the others at different diſtances; and the ſhorteſt were only two or three feet: their diameters were alſo of very different ſizes. Among all theſe diverſities, there was a remarkable regularity. The height of the walls, and of the columns, both of thoſe that are placed againſt the walls, and of thoſe which deſcended from the top, provided they deſcend low enough, is divided into two unequal parts, by a cord which runs through the whole; and is in the ſame horizontal plane raiſed about four feet above the floor. All that is above the cord, is more equal, and uniform, and leſs rugged than that which is below. From the cord, the columns grows thicker to a certain diſtance; after which they grow leſs. In this ſwelling, M. *Maraldi* found the circumference of the columns to be thirty inches; whereas it is only twenty-two above the cord. This floor of the grotto is unequal, and formed by broad and thin ſlabs of marble, placed one upon another; and ſometimes in ſuch a manner, that they make little arches, which, by walking upon them, ſink in, and break.

As there is near this place a river, the waters of which have a ſulphurous taſte, and ſmell, M. *Maraldi* believes, that theſe waters in filtrating thro' the earths, may have brought away ſome clay, or ſand, which being mixed with ſome ſulphur, may have formed all the petrifications of the grotto; for he obſerves, that the ſulphurous waters of *Tivoli* have always a number of little ſtones; the aſſemblage of which forms a ſort of *Travertin*; and that probably theſe waters have produced them, ſince the common opinion of the workmen is, that this *Travertin* increaſes ſenſibly. Some finer ſands which have been brought at firſt, have made the petrefications which are above the cord, more equal, and more perfect; afterwards, ſome coarſer ſands which have paſſed thro' the paths, which the firſt had opened, perhaps alſo mixed with too much water, to give it an eaſier paſſage, have made the lower petrifications, leſs uniform, and not ſo well wrought.

The grotto of *Antiparos*, which was ſpoken of by the late M. *Tournefort* in the memoirs of 1702*, was alſo full of pieces of marble; but they came out of the earth, and raiſed themſelves upwards. And if, as as we have ſaid in the hiſtory of 1708, this grotto in the hypotheſis of M. *Tournefort* was a garden, of which the pieces of marble were the plants, this of *Foligno* will be alſo a garden, but inverted ſince the plants grow from its arch, and deſcend in this reſpect like coral.

IV. *On the reduction of the depth of ſnow in melting.*

According to M. *de la Hire*'s obſervations, ſnow, when melted, is reduced always to the fifth or ſixth part of the height that it had. The night between the 13th and 14th of *February*, this year, there fell ſome ſnow, which was reduced to $\frac{1}{12}$th of its height; that is to ſay, in melting it diminiſhed as much again as uſual: the reaſon of it is, as M. *de la Hire* has obſerved, that it was very fine, very thin, and all in ſmall flakes, and exceſſively dry; which ſupporting themſelves, one upon another, occupied a great deal of ſpace. Becauſe of this dryneſs, it faſtened but little upon the houſes; and that which fell upon the north-ſide, from whence the wind came, was entirely carried away, although there fell ſix or ſeven inches of ſnow.

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* Vol. I. p. 419 of this abridgment.

V.

V. *Of the colours of bodies melted by the burning-glaſs.*

M. *Homberg* has ſaid, that ſuch ſubſtances as gold, ſilver, *&c.* which being in fuſion at the focus of a burning glaſs, appear to the naked eye, only under the colour of light; and with a prodigious brightneſs, are ſeen with their natural colours, if they are viewed through a ſmoaked glaſs.

VI. *Of a glew for agat.*

M. *Homberg* has proved, that the glew of cheeſe, which is proper for glaſs, is of no uſe for agat; and that it muſt have the varniſh of china.

VII. *Of a ſalt found in the waters of* Arcueil.

In *November* 1710, M. *de la Hire* the ſon, having a mind to make ſome experiments, had with the water of *Arcueil* filled a bottle, in which there had been wine; but it had been rinſed with two or three waters; he put a piece of lead in it as big as a nut, and afterwards ſtopped it well with a cork. He left it without touching it, in a place where the ſun did not ſhine, and where there was no fire made. In *January* following, he looked at his bottle, and perceived that upon the convex part of the bottom of the bottle, there was a little white body, as big as a pin's head, and and ſome days after, not having removed the bottle, he ſaw that it was a grain of ſalt of a cubic figure, and that it began to form ſeveral others round it. They continued forming themſelves till the end of *May*, when there was about twenty of a middling ſize, and as many ſmall ones. M. *de la Hire* drew ſome of them out of the bottle without emptying it, and he found they had the figure of the ſea-ſalt, and a little of its taſte. Having kept them ſome days wrapt up in a paper, he ſaw that they were become white inſtead of tranſparent, as they were before, that they were almoſt all reduced to powder, and calcined of themſelves; and that thoſe which were not ſo, eaſily broke, and became a very fine white powder.

As the water of *Arcueil* produces a ſtony cruſt in the channels where it runs, we might believe, that the matter found in the bottle was of the ſame nature, but it had a taſte, and calcined in the air, two qualities which the other has not.

We know that lead diſſolves in vinegar, and M. *de la Hire* ſuſpected, that ſome acid particles of the wine which had not been taken away in waſhing the bottle, might have acted upon the little bit of lead that he had put in, and looſened theſe little white grains from it; but if they had been lead, they would eaſily have incorporated with an oil, as that of nuts, which they did not do at all.

There is therefore more probability, that they were ſalt, altho' it is pretty extraordinary, that they ſhould be thus formed in ſo pure a water as that of *Arcueil*. In emptying the bottle, M. *de la Hire* ſaw, that ſome bits of threads were hardened by this ſalt, and that the reſt were rotten.

VIII. *Of a ſurpriſing ſwiftneſs in the motion of an almoſt inviſible inſect.*

M. *Deliſle* has obſerved, that a fly, ſo ſmall as to be almoſt inviſible, run over a paper of almoſt three inches, in half a ſecond. It was ſo ſmall, that its feet might be reckoned to apply themſelves ſucceſſively upon the whole ſpace that it run over ; and as it appeared to M. *Deliſle* that they might be $\frac{1}{5}$ of a line in bigneſs; it made in the ſpace of a line, fifteen ſteps, or fifteen motions ; and conſequently it made 540 in the ſpace of 3 inches. How nimble muſt it be to remove one foot above 500 times in $\frac{1}{2}$ a ſecond, or more than 1000 times in one of the common pulſations of our arteries! It is true, that with a magnifying glaſs, this inſect appears to have two wings, but it is not perceived to uſe them.

IX. *Obſervations on ſome puppies killed haſtily.*

M. *Littre* ſays, that having ſuddenly, and at one ſtroke, cut off the heads of ſucking puppies, he found the ſtomachs full of ſour and coagulated milk. Now there was not any conſiderable alteration made in it, ſince the death of the animal was ſo quick ; and conſequently it appears, that the milk was ſoured by a natural ferment of the ſtomach ; and that it is this ferment which makes the digeſtion, which ſome ſkilful moderns make to depend entirely upon the *trituration* of the membranes of this bowel.

M. *Littre* had another deſign in this experiment ; he had a mind to ſee if the water of the *Pericardium*, and that of the ventricles of the brain, which we commonly find in dead bodies, were not produced, as ſome maintain, by the approach of death, by ſickneſs, by agitations, *&c.* Theſe puppies ſo haſtily killed, were proper for reſolving the queſtion. They had water both in the *pericardium*, and in the ventricles of the brain, and conſequently they muſt have ſome natural uſe.

X. *On windmills.*

The perfection of windmills, that we have boaſted of in the hiſtory of 1701*, muſt be underſtood only of the two points there mentioned. The axis of the mill which carries the ſails, muſt be put exactly in the direction of the wind, and the ſails muſt be oblique with regard to this axis, and make with it an angle of almoſt 55°. The firſt point is always obſerved in the practice; the ſecond is often wanting; and M. *Parent* has obſerved, that about *Paris*, the angle which ought to be 55° is 71 ½, which receeds too far from what is preſcribed by the theory of mechanicks.

Beſides this well known defect, which cannot be imputed to the theory, it is very poſſible that there may be others in the machine which we do not know, becauſe the geometricians have not examined them enough. They put four ſails, and they make them rectangular; the proportion of theſe rectangles, is commonly 32 feet length of ſail, from the centre of the *axis*, or *arbor*; and 27 feet length of cloth, to 7 in breadth. But how are we ſure that theſe are the number of ſails, and the figure, and the proportion which agree the beſt with the deſign of the machine? We might even conſider alſo the poſition of the ſails, of which the ſmalleſt dimenſion, or the breadth, is put to the ſide of the *axis*, without any ways doubting whether the other dimenſion ſhould not be put there. However, nothing of all this is demonſtrated, and M. *Parent* comes at laſt to diſſipate this uncertainty, and fix all the views that can be had upon this ſubject.

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* Vol. I. p. 258.

The whole force of a windmill depends on the impreſſion of the wind upon its ſails. We ſhall not determine the number of them; but conſider only one, of which the figure, the proportion, and poſition, ſuch as we have juſt explained, ſhall remain undeterminate. We muſt only ſuppoſe the *axis* to be in the direction of the wind, and that the ſail makes with the *axis*, an angle of 55°, ſince theſe are already two advantages that we are ſure of, and two truths, of which we are ſatisfied. The impreſſion of the wind upon one ſail, is as much greater as the ſurface of this ſail is greater, ſince there are a greater number of points impelled by a force, which in itſelf, is equal. But as the ſail is faſtened to the *axis*, the ſame wind acts upon the different parts of the ſail, with ſo much more advantage, as they are farther diſtant from the centre of the *axis*, becauſe a greater diſtance from this centre, which is the fixed point, is a greater lever, by which the wind acts: thus we may look upon the whole length, or height, of the ſail, as a ſeries of levers continually increaſing; among which there is neceſſarily a mean, which ſo compenſates the diſadvantage of the ſmall, and the advantage of the great ones, that if the wind acted by this lever alone, it would act as much as by all the reſt together, and therefore the *centre of the impreſſion* of the wind upon the ſail, is at the extremity of this lever, according to the idea that we have given of theſe ſorts of centres in the hiſtory of 1702, and 1703.

The action of the wind upon the different points of the ſurface of the ſail, being of itſelf equal, it is the ſame thing as if we ſhould conſider them as equal weights, but unequally diſtant from a fixed point, and acting by unequal levers. Now in this caſe, the point where all their action would re-unite, or to ſpeak more accurately, the point in relation to which the product of the weights, by their levers, would be equal on both ſides, would be the centre of gravity. Therefore here the centre of gravity of the ſail, faſtened to the *axis*, and the centre of impreſſion of the wind upon the ſail, are only the ſame thing.

The farther the centre of gravity of the ſail ſhall be from the centre of the *axis*, the longer will the lever of the wind be, and the more advantageouſly will the wind act. Beſides, its force is abſolute, which is as much greater, as the ſurface upon which it falls is greater; and conſequently the whole force of the action of the wind, is a product of the ſurface of the ſail, by the diſtance of the centre of the *axis* from the centre of gravity of the ſail.

We know, that according to the figure, and more exactly according to a geometrical nature of a ſurface, the centre of gravity is there placed at different diſtances from the ſame fixed point, with regard to which we conſider the different levers of its different parts. Thus the wind may act with more force upon a ſmall ſurface, than upon a large one; if in recompenſe it acts upon the ſmall by a longer lever; and in general, the lever of the wind being variable, according to the figure of the ſails, we muſt neceſſarily make it enter into the examinations of the force of the wind. We ſuppoſe for the ſail, a length, or height, which ſhall always be the ſame example, or 32 feet, let the ſail be filled or not, that is to ſay, let the cloth begin, or not begin, from the *axis* of the mill.

M. *Parent* gave at firſt a figure to the ſail that had never been ſeen; he would have them a ſector of an *ellipſis*; the centre of which, ſhould be that of the *axis*, or *arbor*, of the mill; and the ſmall *ſemi-axis* the height of 32 feet; and for the great, it neceſſarily comes afterwards by the ſuppoſition, which always ſubſiſts, that any ſail whatever is inclined to the *axis* of the mill by 55°. We ſhall not reſt upon this point, which is too geometrical. The elliptical ſail is full: but to have its centre of gravity, it muſt be known what portion of an *ellipſis* it is, if this ſector is $\frac{1}{2}$, or $\frac{1}{4}$, or $\frac{1}{6}$, or which is the ſame thing, if we would allow 2, or 4, or 6, ſails to the mill; and this is the reaſon.

The centre of gravity of an elliptical ſector, is the ſame as that of the correſponding ſector of a circle deſcribed upon the little *axis* of the *ellipſis*. Now to have the centre of gravity of a circular ſector, we muſt have that of the arch of this ſector; and the whole depends upon well underſtanding what the centre of gravity of an arch of a circle is.

We muſt conceive this arch with its cord, in proportion to which all the points of the arch are as equal weights, acting by levers, which are lines drawn from each of theſe points, upon the cord parallel to the *radius*, that cuts the cord in half. The part of this *radius* contained between the cord, and the arch, is the verſed ſine of the arch. This ſine is the greateſt of all the levers; and it is this, by which the ſummit of the arch acts. All the others to the right, and left, continually diminiſh, till they come to nothing. It is viſible, that a mean lever will be ſmaller than this verſed ſine; and will be a part of it; one extremity of which will be at the *vertex* of the arch, and the other will be the centre of gravity of the arch. The ſmaller an arch is, the ſmaller is its verſed ſine; and conſequently alſo its mean lever, part of this ſine, and reciprocally. It remains to ſee according to what proportion the mean lever is part of the verſed ſine.

A mean lever among many ſmall, and many great ones, will be as much greater, as the number of the ſmall ſhall be leſs, in proportion to that of the great ones. Now in conſidering the different arches of a circle with their cords, we ſee evidently, that the greater an arch is, the leſs is the number of theſe ſmall levers, and the larger is the number of the great ones. Therefore, the larger an arch is, the greater is its mean lever; and is leſs exceeded by the verſed ſine; that is to ſay, it is a greater part of this ſine. The mean lever of the ſemi-circumference, for example, will be a greater part of the *radius*, which is then the verſed ſine, than the mean lever of any other arches leſs than 180, compared in like manner to its verſed ſine.

The greater an arch is, the greater it is in proportion to its cord, which appears manifeſtly in this, that an infinitely ſmall arch is equal to its cord; and that therefore the arch of 600, which has the ſemi-diameter for a cord, is not ſo great in proportion to this ſemi-diameter, as the arch of 180; or the ſemi-circumference, in proportion to the diameter; for theſe two terms are ſufficient to eſtabliſh the ſpecies of the progreſſion. Therefore the greater an arch is in proportion to its cord, by ſo much is the mean lever of an arch, a greater part of its verſed ſine.

The greater part of its verſed ſine, the mean lever of an arch is, the more does the centre of gravity of the arch approach to the centre of the circle, or which is the ſame thing, the more does the diſtance of theſe two centres diminiſh; and it is the ſmaller in proportion to the *radius.* Therefore the greater an arch is in proportion to its cord, the greater is the radius in proportion to the diſtance from the centre of the circle, to the centre of gravity of this arch; and it is this exact proportion which is determined by geometry. We ſee by this, that the diſtance of the centres of gravity of the arches, from the centre of the circle, varies according to the proportion of the arches to the cords which is continually unequal.

It is eaſy to imagine what is the centre of gravity of a circular ſector. We may conceive them divided into circular zones, which will act with relation to the centre of the circle, by greater or leſs levers, according as they ſhall be more or leſs diſtant from this centre. They will have a mean lever, of which the extremity that ſhall be within the circle, will be the centre of gravity of the ſector. It is demonſtrated, that after we ſhall have determined the centre of gravity of the arch of this ſector, if we take $\frac{2}{3}$ of the diſtance from the centre of the circle to this centre, it is there that the centre of gravity of the circular ſector is.

Since the centre of gravity of a ſector depends upon that of the arch, the ſame conſequences recur, the greater a ſector ſhall be, the more will its centre of gravity approach to the centre of the circle; and that in the ſame proportion, according to which, a greater arch exceeds its cord. Therefore if a power act upon a ſector which has the centre of the circle for a fixed and immovable point, and if the whole action re-unites in the centre of gravity of this ſector, the power will act by a lever, as much ſhorter as the ſector ſhall be greater; the ſame muſt be ſaid of elliptical ſectors, as of circular, ſince their centre of gravity is the ſame.

To calculate the force of the wind upon one ſail of a mill, which ſhould be an elliptic ſector, M. *Parent* has therefore been obliged to determine what this ſector is, becauſe of the variation of the centres of gravity; or which is the ſame thing, of the levers of the wind. He at firſt took a ſector which was $\frac{1}{4}$ of the ellipſis; and conſequently has allowed to the mill 4 ſails, which would receive all the wind, and not looſe any of it, as the common ſails do. Theſe 4 large ſurfaces multiplied by the lever of the wind upon one of them, expreſs the whole force of the wind to make the machine move, or the force of the machine put in motion.

The ſame manner of reaſoning applied to a common wind-mill, the ſails of which are rectangular, and the height about 5 times greater than their breadth, ſhews that the elliptic mill has almoſt 7 times more power, which is a prodigious advantage, and certainly deſerves to have the common practice entirely changed for it, were it but eaſy to change ſo common a practice. One would not have expected that the theory could have diſcovered ſo great an error in an eſtabliſhed cuſtom; but it is true that elliptical ſails would not eaſily at preſent themſelves to the mind of the firſt inventers.

A mill with 6 elliptic ſails would ſtill be better for the power, than one with 4. It would have only the ſame ſurface, ſince theſe 6 ſails would contain the whole ſpace of the *ellipſis*, as well as the 4 of the other, but its force would be greater almoſt in the *ratio* of 245, to 231. The reaſon of this augmentation is viſible. A ſector that is $\frac{1}{6}$ of a circle, is leſs than that which is $\frac{1}{4}$; and conſequently the wind acts upon the firſt by a longer lever.

As the difference of 245, to 231, is trifling; and as beſides the wind might be hindered in ſix ſails, and be reflected from one to the other, in ſuch a manner as would trouble their motion, I believe we had better keep to the mill with 4 elliptical ſails.

If we would have it with two, each of which ſhould be a *ſemi-ellipſis*, we ſhould find the ſame ſurface alſo; but the force would be diminiſhed almoſt $\frac{1}{3}$ with relation to the mill with 6 ſails, becauſe the greatneſs of the ſectors would very much ſhorten the lever of the wind.

Elliptical ſails to a mill would be ſomething ſo new, that the common uſe of them would hardly be adopted. Thus M. *Parent* thought he ought to ſearch for the moſt advantageous rectangular ones, that is, thoſe where the product of their ſurface, by the lever of the wind, would be the greateſt. Every body knows, that the centre of gravity of a rectangle is its middle point, and conſequently the lever of the wind is the diſtance of this point from the centre of the *axis*.

M. *Parent* inſcribes in the elliptical ſail of a mill with 4 ſails, a rectangle of which he does not determine the dimenſions, and which conſequently repreſents all that can be inſcribed there, after which, the geometrical rules *de maximis & minimis*, determine this rectangle to be the moſt advantageous of all. That which comes by this means, is yet very different from the common uſe. The breadth of the rectangular ſail muſt be almoſt double its height or length; whereas the height is commonly almoſt 5 times greater than the breadth. We ſee alſo, that ſince we call height, or length, the dimenſion that is taken from the centre of the *axis*, the greateſt dimenſion of the new rectangular ſail, will be turned on the ſide of this *axis*, quite contrary to the poſition of the old ſails. They have been miſtaken in all this to a ſtrange exceſs.

The force of a mill with 4 elliptic ſails, would be to that of a mill with 4 new rectangular ſails, pretty near, as 23 to 13, which always preſerves a great advantage to the elliptic mills.

If we compare together mills with two, with four, and with ſix, new rectangular ſails, and the moſt advantageous that can be, ſuppoſing this number of ſails, and always inſcribed in the correſponding elliptic ſectors, we ſee that thoſe which have feweſt ſails, have moſt ſurface, and leaſt force. The force diminiſhes becauſe the height which increaſes brings the rectangles nearer to the centre of the axis, and conſequently alſo their centres of gravity, and ſhortens the lever of the wind, according to a greater proportion than the ſurface increaſes. The force of the mill with 6 ſails, is to that of the mill with 4, much as 14 to 13, which perhaps ought not to hinder the mill with 4, from being prefered, becauſe of its greater ſimplicity. Its force with relation to that of the mill with 2 ſails is almoſt as 13 to 9.

After this it is eaſy to calculate the force of the common mill, where we muſt ſuppoſe the height of the ſail always much greater than the breadth. But in this ſuppoſition, whatſoever proportion the breadth has to the height, let it be $\frac{1}{3}$, $\frac{1}{4}$, or $\frac{1}{5}$, we find always the force of the mill much ſmaller than if it had the new rectangular ſails, and much more if they had elliptical ones. And alſo the force of the common mill continues to diminiſh, juſt as its ſails are ſmaller in proportion to its height, ſo that the weakeſt of all thoſe we have marked, is that where this breadth is $\frac{1}{5}$ of the height, and yet this is the moſt uſed; ſo obſtinate does the common practice ſeem in being miſtaken.

The uſefulneſs of all this theory of M. *Parent's* is eaſily perceived. A ſtronger mill will turn faſter, and with a leſs wind, and will diſpatch more work. In a low place, and where for want of wind it would be uſeleſs to conſtruct an old mill which would be too weak, they may conſtruct a new one. We ſhall have, if we pleaſe, a mill of a leſs height of ſail, which, nevertheleſs will be equal in force to an old one; and we ſhall know exactly how much we ſhall lower its ſail; preſerving this equality. When we ſhall know the effect that is required to the mill, that is the force that we would give it, in pounds, it will be eaſy to find by calculation the number of ſails, their figure, proportion, and even the varieties, and different combinations, that theſe things may have always the ſame effect. The trouble of diſcovering the true principle is always rewarded by a great number of eaſy conſequences.

XI. *On the manner in which ſeveral ſorts of ſhell-fiſhes faſten themſelves to certain bodies.*

In treating of the progreſſive motion of ſeveral kind of ſhell-fiſh, in the hiſtory of 1710*, we have ſpoken of the almoſt perpetual immobility of ſome of them; for we cannot treat of their progreſſive motion without ſaying that the greateſt part of them have hardly any; and in this reſpect partake more of plants, than of animals. There are even ſome which abſolutely never ſtir from the place where (if I may uſe the expreſſion) they

* Vol. III. page 321 of this abridgment.

have taken root. We are at preſent going to explain their immobility according to M. *de Reaumur*, the author of all theſe obſervations.

The *patella*, or limpet*, fixes itſelf by a very flat baſe to ſtones, and even very ſmooth ones; and is faſtened there ſo ſtrongly, that being put in a ſituation where this baſe and the ſtone were vertical, it required a weight of 28 or 30 lb. to make it let go. We muſt obſerve that this baſe, which is elliptical, has but one inch in its greateſt diameter. From whence can this great ſtrength proceed? it is not very probable, ſeeing the ſmoothneſs of the two bodies, that the baſe of the limpet, how muſcular ſoever, ſhould be ſufficiently inſerted into the imperceptible inequalities of the ſtone; and indeed this inſertion would not have any great effect in the vertical ſituation. Beſides, M. *de Reaumur* is aſſured by deciſive experiments, that this ſhell-fiſh faſtens itſelf ſo ſtrongly to the ſtone, by the means of a glue which proceeds from it; and that the action of the muſcles of its baſe, which we might add thereto, has no ſhare in it.

This glue is yet more remarkable in the ſea-nettles*, which are not leſs cloſely fixed to the ſtones. Theſe animals are neither covered with ſcales nor ſhells, and their ſkin is not a membrane, or texture of ſolid fibres; it is only a covering of glew, which diſſolves very ſoon in ſpirit of wine, whilſt the reſt of the body of the animal remains entire, and without alteration.

The 1520 legs of the ſtar-fiſh* do not ſeem to be given it ſo much for walking, as for not walking. They are very ſoft, and ſerve it to ſtick to the neighbouring bodies, in ſuch a manner, that if one had a mind to diſengage it from them, we ſhould only break them.

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* Vol. III. page 321 of this abridgment.

The ſea-muſcles have a much more ſingular manner of faſtening themſelves. They throw out ſome threads of the bigneſs of a thick hair, at moſt about 3 inches long, and ſometimes to the number of 150, with which they ſeize all that ſurrounds them, and generally the ſhells of other muſcles. They are thrown on all ſides; and they hold by them, as by cords of different directions. M. *de Reaumur* has not only ſeen that they ſpin them, and that when they have been cut off, that they have ſpun others; but he has diſcovered the curious particulars of the mechaniſm that they make uſe of.

The *pinnæ marinæ*, another ſort of ſhell-fiſh, fix themſelves alſo in another ſituation, by threads much finer than thoſe of the muſcles, but in a much greater number; there are fine works made of them, whereas thoſe of the muſcle are good for nothing. There are no *pinnæ marinæ* on the coaſts of *Poictou*, where M. *de Reaumur* has obſerved; but it is a ſtrong preſumption that they ſpin alſo. Theſe will be the ſilk worms of the ſea, and the muſcles will be the caterpillars.

Laſtly, the worms which are commonly called worm-ſhells, becauſe being otherwiſe pretty like the earth-worms, they are incloſed in a round tube, of the ſubſtance of a ſhell, make themſelves a habitation which they never quit, by fixing their tube on a ſtone, or hard ſand, or ſome other ſhells. This tube exactly follows the circumvolutions of the ſurface where it is glued, riſes, or ſinks with it, *&c.* It winds alſo without being obliged to it by this ſurface; and becauſe it ſeems to have followed the natural motion of the worm. All this explains itſelf in M. *de Reaumur*'s ſyſtem, who pretends that this tube, as well as the ſhells of the ſnails, is formed of a glutinous matter which comes out of the body of this little animal.

Another ſort of ſea-worms, which probably tranſpire leſs of this matter, make their tube only of grains of ſmall ſand, and little fragments of other ſhells which they unite together by their glue; and this little building of pieces brought together, is however neatly enough made.

It is by means of this very glue, that the oyſters ſtick themſelves to the rocks, or to one another; and laſtly, this is the univerſal cement which nature uſes whenever ſhe has a mind to build in the ſea, or to ſecure any thing there againſt the perpetual and violent motion of the waters. The moſt ſimple means properly employed are the moſt efficacious.

An explanation of the figures.

Plate II. *fig.* 1. repreſents a *patella*, called in *Engliſh*, a *limpet*, *ſlither*, or *papſhell*, faſtened to a ſtone. It ſhews only the ſhell which covers it. The letters BBB mark the circumference of the baſe of this ſhell; S is its *vertex*. There are different flutings which go from the *vertex* S to the baſe.

Fig. 2. is a *limpet* detached from the ſtone, and put into a reverſed poſition T; is the head of the animal; CC are two horns placed near the head; P is the fleſhy baſe of the animal. It is this baſe P that is applied to the ſtones, and ſticks to them. Its ſurface ſeems rugged, and as it were ſhagreened. All theſe inequalities are formed by an infinite number of different little veſicles.

Fig. 3. is a ſea-muſcle repreſented open. The muſcle MM, which ſerves to ſhut the ſhell, has been cut.

AB is a part of the muſcle which reſembles the tongue of an animal. It is the inſtrument that moulds the threads formed by the animal. A is the origin, baſe, or root of this inſtrument. B is its point, or extremity.

AI is a ſtreak, or rather cleft, of which the two edges are applied to each other, and it forms a canal on the inſide. This cleft divides the ſpining inſtrument into two equal parts. From A to I we muſt obſerve ſome circular, or rather tranſverſe fibres: they ceaſe in I.

AF is a part of the bundle of threads which ſerve to faſten the muſcle; theſe threads have been cut at F, that the figure might be leſs confuſed; and alſo to ſhew how ſhort they were, when we had cut them with a pair of ſciſſars, introduced into the ſhell on account of this figure 3; we ſhall obſerve, that the mouth of the muſcle is at C; it is formed of two pretty thin membranes, which ſeem to be applied to each other. We do not ſee this mouth open, unleſs we take care to open it: its breadth is HH. This mouth is a ſort of funnel, very much flatted, and terminates in a duct, which reaches quite to the *anus*. It is nouriſhed in all probability only by the water and earth; its excrements are of the colour of the mud.

Fig. 4. is a muſcle, which having prolonged its ſpinning inſtrument, marked at preſent LI, feels about to find the ground before it fixes itſelf. This inſtrument appears under a very different form from that which it has in the inaction, as may be ſeen at AB, *fig*. 3.

VVV are *tubuli vermiculares*, or *worm-ſhells*, ſtuck upon the ſhell of a muſcle. Theſe worms grow indifferently upon all ſorts of bodies, as ſtones, ſand, and other ſorts of ſhell.

Fig 5. is compoſed of 1. a muſcle G, which is faſtened to a ſtone by different threads, DDD, *&c.* The baſe DD of theſe threads, is three or four times more in diameter, than the reſt of the thread. We ſee at G a little end of the tendon, or big thread, to which all the ſlendereſt threads are faſtened. 2. In *fig.* 5. there is a muſcle N, which after having darted the 2 threads NQ, NQ, actually darts a third NT. T is the place where the end of this thread ought to be ſtuck. We may obſerve, that the ſpinning inſtrument is thicker there than towards the point; and that it forms a ſort of heel.

Fig. 6. is the half of a muſcle, where the ſpinning inſtrument is however left entire. We ſee there two of the four muſcular ligaments, which hold it. RS is one of thoſe which faſten it toward the *vertex* at S. ZX is one of thoſe which faſten it toward the baſe at Z.

Fig. 7. is a ſpinning inſtrument detached. KP is the cleft, or canal, in which the liquor which becomes thread, paſſes. This canal ceaſes at P, the part PO, where it does not reach, is thinner than the reſt of the baſe. At the root K of the ſpinning inſtrument there appears a hole K, which is the reſervoir, wherein the liquor is gathered that riſes in the ſpinning inſtrument, in the ſame hole K is lodged one of the ends of the tendon, or big thread of *fig.* 9. to which all the ſlender threads are faſtened.

Fig. 8. is the ſpinning inſtrument ſeen behind. There are two pieces of muſcular ligaments MM, which ſerve to faſten it. Theſe pieces are parts of the ligaments, ſuch as the ligament marked ZR, *fig.* 6.

Fig. 9. AB is the tendon, or big thread, to which all the reſt are faſtened, as the figure repreſents. In ſeveral muſcles it is much ſhorter than it appears here; but there are ſome where it is bigger. Its extremity A is faſtened in the hole K of *fig.* 7. or as it is ſeen at A, *fig.* 3. All the threads, which the muſcles have formed with me, were faſtened near A, which inclines me to believe that this tendon, or big thread, grows like our hairs; and that the ſlender threads, which at firſt were faſtened at A, are faſtened at B by the croſſing of big threads.

Fig. 10. is a muſcle repreſented in the ſtate in which it is when it reſpires the water. CD is the aperture by which it reſpires. The canal by which it throws out its excrements, opens into the ſame aperture CD. The outlet of this canal, or the *anus* of the muſcle, is at C: the excrements, which come out of it, ſeem to be meer earth, a ſort of mud. They have a fluting thro' their whole length; I mean that they are made as a portion of a hollow tube. Thence it is evident, that the canal by which they are diſcharged, or at leaſt the aperture thro' which they paſs, is not round as in other animals. RH is the place, or ſpring, which ſerves to open the ſhell. EE are an infinite number of little fleſhy parts very prettily cut, reſembling little cocks-combs. The animal ſhews only when it reſpires the water, they are ſeen alſo at EE. *Fig.* 5. the reſpiration is not ſtopped while it darts the thread.

Fig. 11. is one of the pieces of which the ſhell of a muſcle is compoſed; we may obſerve a little band which covers the inner edge of the ſhell. This band is of a horney kind of ſubſtance, and is faſtened in its natural ſtate to the circumference of the body of the animal.

Fig. 12. is a *pectunculus*, or *cook-fiſh*, faſtened to a ſtone by different threads FFF, the *vertex* of the ſhell is at S; on both ſides of S, is the ſpring which ſerves to open the ſhell; for this ſhell is bivalve, like that of the muſcle. There appear ſeveral flutings, which go from the *vertex* S, to the baſe BB. In different parts the ſhell is ſtuck with points; ſo is that point of the ſhell which is called the ear.

Fig. 13. is a cook-fiſh repreſented open. The thick muſcle MM, which ſerves to ſhut it, has been cut. L marks the *vertex* of the ſhell, and the middle of the ſpring which tends to open the ſhell. T and R are two appendages, which being placed one above the other, form the ear. The appendage T is narrower than the appendage R, ſo that the firſt does not entirely cover the ſecond. They do not apply themſelves ſo exactly to one another, as not to leave a little opening by which a part of the threads came out, which are ſeen in *fig.* 12. HG is the ſpinning inſtrument of the cook-fiſh; GP is the bundle of threads: theſe threads have been cut ſhort at P, for fear of rendering the figure confuſed. They are all faſtened to a common tendon at P, which is faſtened to the origin of the ſpinning inſtrument.

Fig. 14. is a cook-fiſh repreſented in ſuch a poſture as to ſhew the canal VX, thro' which the excrements of the animal paſs. X is the aperture of this canal, or the *anus* of the cook-fiſh.

Fig. 15. is a heap of ſand, in which a great number of *worm-ſhells* were lodged. On the upper ſurface of this heap of ſand is the out-let of all their tubes; and on one of the ſides, as at BC, may be diſtinguiſhed, the length, roundneſs, and curvature of theſe tubes.

Fig. 16. is one of the ſand worm-ſhells, repreſented almoſt in its natural bigneſs.

Fig. 17. is the ſame worm ſeen with the microſcope. The extremity of the head is the flat ſurface ſeen at T. This extremity, the circumference of which is round, is ſometimes made like a horſeſhoe, when the animal opens in O. NNN are the fins of the worm. HH, II, EE, are three rows of little fleſhy hooks. Q is the tail of the worm.

XII. *On a new purple.*

There are not only more things found in theſe laſt ages than there have been old ones loſt, but there can hardly be any thing loſt that we are not contented ſhould be ſo. For in ſhort we need only ſearch in the boſom of nature, where nothing is annihilated; and it is alſo a great ſtep toward recovering it to be ſure that it may be found. The purple colour, formerly ſo much in eſteem, that it made among the *Romans* one of the principal marks of dignity, either has not been abſolutely loſt, as ſome think, or at leaſt has been recovered within theſe 30 years, by the royal ſociety of *England*. One of the ſhell-fiſhes which furniſhes it, and is a ſpecies of *buccinum*, or *whelk*, is common on the coaſts of that country.

Another *buccinum* which alſo gives the purple tincture, and which probably is one of thoſe which *Pliny* has deſcribed, as having that uſe, is found upon the coaſts of *Poictou*, and M. *de Reaumur* having a mind to ſtudy it particularly, diſcovered another purple which he did not ſeek for, and which in all probability was unknown to the ancients, though of the ſame ſpecies with theirs.

The *buccinums* of *Poictou* which yield the purple, are commonly found gathered about certain ſtones, or ſand, covered with oval grains, about 3 lines long, and a little more than a line thick, full of a white liquor, a little yellow, pretty much like that which is drawn from the *buccinums* themſelves, and which after ſome alterations, acquires the purple colour. By M. *de Reaumur*'s experiments it is not likely that theſe grains are the eggs of the *buccinum*, nor are they the ſeeds of any ſea-plants; they muſt therefore be the eggs of ſome fiſh. They do not begin to appear till autumn.

Theſe grains cruſhed upon a white cloth, at firſt only make an almoſt imperceptible yellow, but in 3 or 4 minutes they give it a very fine purple red, provided that this cloth be expoſed to the open air: for what is very remarkable, and which ſhews of what extreme delicacy the generation of this colour is, the air of a room, even though the windows ſhould be open, would not be ſufficient. The tincture of theſe grains fades a little by a great many waſhings.

M. *de Reaumur* has diſcoved by ſome experiments, that the effect of the air upon the liquor of the grains, conſiſts not in its carrying off ſome of its particles, nor in giving it new ones; but meerly in agitating it, and changing the order of the parts which compoſe it. We have in the cochineal a very fine red colour, but good only for wool; and is of no uſe, either for ſilk, or linnen. The *carthamus*, or *ſaff-flower*, gives a fine ſcarlet and crimſon, but it is only to ſilk. We may find in cultivating the grains of M. *de Reaumur*, the fine red that we want for linnen, which perhaps will ſurpaſs the red of the *Indian* linnen which is not fine.

M.

p. 112. IV.

Fig. 1.

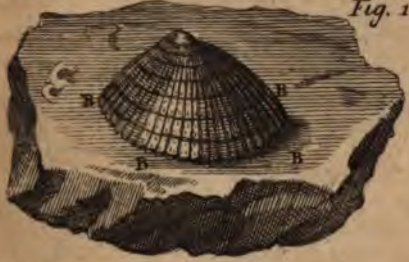
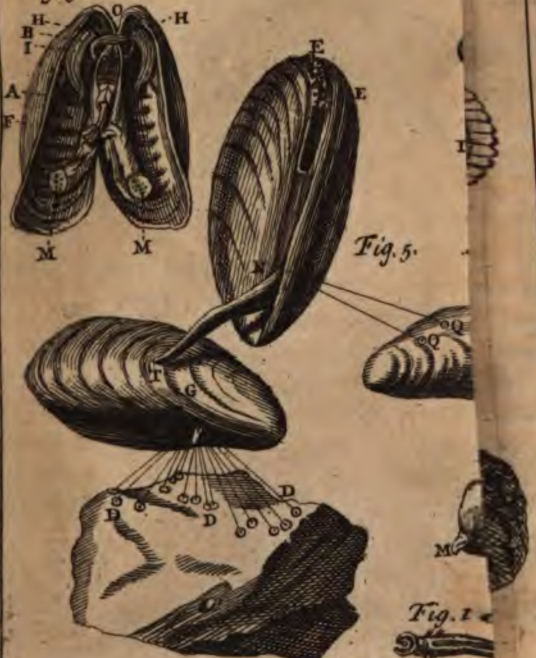


Fig. 3.



nde fc.

ſurpaſs the red of the *Indian* linnen which is not fine.

M. *de Reaumur* has not failed to compare his new purple with that which is drawn from theſe *buccinums* of *Poictou.* The *buccinums* have at their collar, for we may allow them to have one as well as the ſnails, a little reſervoir, called improperly by the antients, a vein, which only contains one great drop of yellowiſh liquor; the linnen that is dyed with it being expoſed to a moderate heat of the ſun, acquires at firſt a greeniſh colour, afterward a lemon colour, a brighter green, then a little deeper, from that a violet, and at laſt a fine purple. This is done in a few hours; but if the heat of the ſun be very ſtrong, the firſt changes are not perceived, and the fine purple appears all at once. A great fire produces the ſame effect, only a little ſlower, and does not produce ſo perfect a colour. Without doubt the heat of the ſun being much more ſubtle than that of a wood fire, is moſt proper to agitate the fineſt particles of the liquor. The open air acts alſo, tho' leſs quick, upon the liquor of the *buccinums*; eſpecially if it is diluted with a good deal of water; from whence M. *de Reaumur* conjectures, with great probability, that the liquor of the *buccinums*, and that of the grains, are almoſt of the ſame nature, except that this of the grains is more aqueous. They differ alſo in taſte; that of the grains is ſalt, and that of the *buccinum* extremely hot, and biting, perhaps becauſe it is leſs diluted with water.

If we would make uſe of them in dying, that of the grain would be much more convenient, and would coſt leſs; becauſe it is very eaſy to draw it from a great number of grains bruiſed all at once; whereas to have that of the *buccinum*, we muſt open the reſervoir of each particular *buccinum*, which requires a great deal of time; or if for expedition we bruiſe the ſmalleſt of theſe ſhell-fiſh, we ſhall ſpoil the colour by the mixture of different matters which the animal furniſhes.

We might find perhaps ſome chymical liquors which would make the purple colour appear quicker, or more conveniently, than the fire, or the ſun, or the open air; and M. *de Reuamur* has already found the coroſive ſublimate, which produces this effect on the liquor of the *buccinums*; but practice, and above all, a practice which ſhould come to make part of a trade, would require a great many other obſervations, and quite new intentions. There is a vaſt difference between a philoſopher who aims at knowledge, and a mechanic who aims at profit.

An explanation of the figures.

Plate III. *fig*. 1. repreſents a ſtone DHHFEED, in which is ſeen a great number of thoſe little grains which M. *de Reaumur* calls eggs of the purple, faſtened as to an arch againſt one of the faces of this ſtone. This face was downward, but it did not touch the ſand. GGG, *&c.* are different places where theſe eggs of the purple are faſtened. The eggs of the purple marked EE, are ſtuck upon other eggs, as the other eggs are ſtuck upon the ſtone.

Fig. 2. is that of an egg of a purple; p is its baſe, the extremity of its pedicle; it is this extremity that is ſtuck againſt the ſtone. p r is its pedicle;

dicle; this pedicle p r ſuſtains a little bottle r b ſtopped at b, with a ſtopple b. d d mark the thickneſs of this bottle.

Fig. 3. is alſo an egg of a purple, where the ſame letters mark the ſame parts as in the preceding figure; but b ſhews the ſtopple looſe; o the aperture of the bottle in which the ſtopple was.

Fig. 4. is a large repreſentation of an egg of a purple. The letters PRB ſhews the ſame things as the letters p r b in *fig.* 2 and 3. The letters III ſhew the manner in which the different drops of yellowiſh liquor are diſtributed in the middle of the clear liquor. As the ſides of the egg are tranſparent they diſcover theſe different liquors.

Fig. 5. is a little *buccinum*, or *whelk*; it ſhews the aperture of the ſhell; and at the edge of this aperture are ſeveral flutings o o o o.

Fig. 6. is a whelk, the ſhell of which differs from the preceding by the coloured ſtripes RR.

Fig. 7. is the ſame whelk as *fig.* 5. which ſhews its head. T is the head, at the ſides of which are 2 horns CC. DDDD mark that part of the ſhell which covers the collar, or upper part of the back of the animal. This part DDDD of the ſhell muſt be raiſed to perceive the little veſſel wherein the purple dye is contained.

Fig. 8. is the ſame whelk with *fig.* 7. with a piece of the ſhell DDDD removed, which ſhews its collar EEEE. Upon this collar, or if you pleaſe, on the back of the animal, appears the little veſſel v v. It is in this veſſel that the purple dye is contained.

Fig.

Fig. 9. is that of the whelk from which *Columna* pretends the purple dye of the ancients was taken. This whelk uſually ſhews 3 horns, the largeſt of which C is in the middle between the 2 ſmaller c c. The ſame whelk may be ſeen in the hiſtory of 1710, with the great horn C in a different poſition.

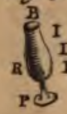


Fig. 4.

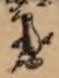
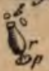


Fig. 2.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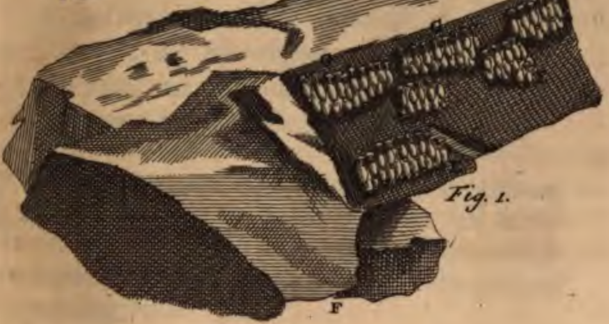
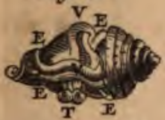


Fig. 1.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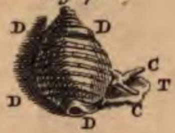
Fig. 7.

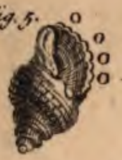
Fig. 5.

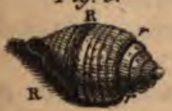
Fig. 6.

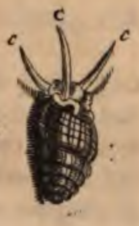
Fig. 9.

J. Mynde sc.

A N
ABRIDGMENT
OF THE
PHILOSOPHICAL MEMOIRS of the ROYAL ACADEMY of SCIENCES at *Paris*, for the Year 1711.

I. *Obſervations on the height of the water which fell at the obſervatory during the year* 1710, *with thoſe on the thermometer and barometer, by M.* de la Hire*.

THESE are the obſervations of the quantity of water, and melted ſnow, that fell at the obſervatory during the whole year of 1710, which have been made in the ſame manner with thoſe of the preceding years. In

	Lin.		*Lin.*
Jan.	12 $\frac{3}{8}$	July	17 $\frac{3}{8}$
Feb.	3 $\frac{1}{2}$	Aug.	37 $\frac{1}{8}$
March	14 $\frac{1}{8}$	Sept.	15 $\frac{3}{4}$
April	17 $\frac{3}{4}$	Oct.	11 $\frac{3}{8}$
May	12	Novem.	21 $\frac{1}{8}$
June	9	Dec.	17

The ſum total of water of the whole year, 188 lines $\frac{3}{4}$, or 15 inches 8 lines $\frac{3}{4}$.

Theſe obſervations ſhew us that the year 1710 has been one of the drieſt that we have had a great while, in compariſon to the 19 inches of water which commonly falls. The year however has been very fruitful in grain, as it always happens in theſe countries, becauſe the greateſt part of the grounds are cool and moiſt.

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* Jan. 9, 1711.

There fell no ſnow at the end of the year, but at the begining of it, about the middle of *January*, it ſnowed moderately; which gave me the opportunity of making the following experiments.

The 10th of *January* in the morning I covered the ball of my thermometer, which is always expoſed in the open tower of the obſervatory, with a very great quantity of ſnow, and after having left it there full 3 hours, I did not obſerve that the ſpirit of wine had changed its height in the tube; it then remained at 27 parts, and it begins to freeze in the country when it is at 32, by which we ſee that the air was not much colder, that in the beginning of the froſt; and altho' the thermometer always riſes from the morning 'till noon, and afterwards it does not change its height for 3 hours, becauſe the degree of cold of the ſnow kept it always in the ſame ſtate, the ſmall increaſe of heat of the air, not being capable of penetrating in ſo ſhort a time, the maſs of ſnow which was about the ball.

But the air being extremely cool till the next day, the 11th of this month, this thermometer being then at 14 $\frac{1}{2}$ parts, which marked a great cold, I repeated the experiment of the preceding day, and there happened the ſame thing, the thermometer remaining at the ſame height, in the ſnow as it was out of it; from whence I conjecture that the cold of the ſnow is not a cold that is proper to it, but that it only takes the degree of cold of the air as it then is, becauſe it is rare enough to leave the air at liberty to inſinuate itſelf by degrees into all its parts; thus the ſnow will contribute nothing to the cold, except the preſerving

ſerving the coldneſs of the air for ſome time in the ſame ſtate.

There was not any thing conſiderable to remark upon the winds, except that *October* 11th, there was a ſort of hurricane, the wind being at S. S. W. without rain.

The thermometer marked the greateſt cold of the year, *January* the 11th, being fallen to $14\frac{1}{2}$ parts, which marks a great cold; but the 12th it roſe again to 27, where it was the 10th and from that time the cold was moderate.

As for the heat, it was alſo moderate during the whole ſummer, the greateſt was mark'd by the thermometer at 61 parts the 3d of *Auguſt* at ſun-riſe, and at $2^h \frac{1}{2}$ P.M. the thermometer was at $71\frac{1}{2}$ parts; thus the cold was greater than the heat, in proportion to the mean ſtate, where it is at 48, but it laſted only one day, as I have juſt related.

My common barometer which is always placed at the top of the hall of the obſervatory, was at the higheſt at 28 inches 3 lines $\frac{1}{6}$ *January* the 3d. with a S. W. which is very extraordinary, for it is commonly rather low than high, when the wind is in the ſouth. It was at the loweſt, *March* the 7th, at 26 inches, 10 lines $\frac{1}{6}$, alſo with a ſouth-wind, and rain. The difference between the higheſt and the loweſt was therefore 1 inch, 4 lines $\frac{1}{3}$ a little leſs than ordinary, which is 1 inch 6 lines.

I alſo obſerved that in the whole month of *February*, when it rained but very little, the barometer was very high as it commonly is; it was alſo the ſame in the firſt half of the month of *September*.

I here again give notice, that when the obſervations are made, they ſhould take care to ſtrike a little againſt the wooden frame, where the tube is fixed, to make the quickſilver run to its true height; for as it always adheres a little to the inſide of the tube, it does not move freely, and ſometimes there is found $\frac{1}{2}$ a line difference between the height at which it appears to be at firſt, and the true height where it ſtops at, eſpecially if the tube be thin.

December 30, I obſerved the declination of the needle of 8 inches long, in placing the ſide of the box againſt the ſame ſtone pillar where I geneally put it, and I found it 10° 50′ towards the weſt.

II. *A compariſon of our obſervations on the height of the rain-water, and on the barometer, which thoſe which* M. Scheuchzer *made at* Zurick, *in* Swizzerland, *during the year* 1710, *by* M. de la Hire†.

M. Scheuchzer has ſent us this year his obſervations of the rain water, and of the barometer and thermometer, which he has made at *Zurick*, as in the year 1709. He found the height of water only 23 inches $\frac{1}{4}$, during the whole year 1710, and he adds that it is leſs than the preceeding year, by 9 inches 2 lines $\frac{3}{4}$, and yet this little height is greater than any of the greateſt that we have obſerved at *Paris* ſince the year 1699.

I have related in the memoir of the preceding year, my conjectures upon what may cauſe this greater height of water in the mountains, and therefore I ſhall ſay no more of them here. We ſhall only obſerve that the height of the rain-water at *Paris* in 1710, was only 15 inches, and almoſt 9 lines, which is much leſs than ordinary; and leſs than the preceding year by 6 inches; which agrees in ſome meaſure with M. *Scheuchzer's* obſervations; and ſhews that at *Zurick*, and at *Paris*, the year has been dryer than ordinary.

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† Aug. 5. 1711.

He adds, that the greateſt height of his barometer was *Jan.* 3, at 26 inches, 9 lines $\frac{3}{4}$, and the leaſt at 26 inches, 0 lines $\frac{1}{2}$, *Dec.* the 25; the difference is therefore 9 lines $\frac{1}{4}$.

I found my barometer alſo at the higheſt the 3d of *Jan.* at 28 inches, 3 lines $\frac{1}{8}$; therefore the difference of the height of the quickſilver the ſame day at *Zurick*, and at *Paris*, was 1 inch, 5 lines $\frac{5}{12}$, or 17 lines $\frac{5}{12}$; from whence we might conclude pretty near, how much higher *Zurick* is than *Paris*, if our barometers agreed.

The leaſt height of quickſilver which I found, was 26 inches, 10 lines $\frac{5}{6}$; therefore the difference of our leaſt heights will be 10 lines $\frac{2}{3}$, which is very different from the preceding; the days were alſo very different; and the 15th of *Dec.* which was the day of the obſervation at *Zurick*, my barometer was at 27 inches.

As for the heights of his thermometer, I cannot compare them with mine, for they muſt have been rectified by one another.

III. *Experiments to know whether the ſtrength of cords exceeds the ſum of the forces of the threads which compoſe them, by M.* de Reaumur*.

We are prejudiced in believing, that a cord compoſed of different threads twiſted together, has a force which ſurpaſſes the ſum of the forces of all the threads which compoſe it. I mean that if, for example, we make a cord with 6 threads, each of which will bear a weight of 5 lb. without breaking, it is generally thought, that that the cord made with theſe 6 threads, will bear a weight above 30 lb. ſeveral learned men are agreed with the vulgar in this, as I had the opportunity of ſeeing, by the objections which were made to me by ſome of the moſt illuſtrious perſons of the academy, upon a paſſage in the memoir where I examined the ſilk of ſpiders. This paſſage treated of the ſtrength of the filaments of ſilk. A ſkilful geometrician pretended even to have the demonſtration of the proportion, in which the twiſting is convenient; and I ſhall often have occaſion hereafter for the proportion, in which the twiſting increaſes the ſtrength of the cord above the ſum of the forces of all its threads.

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* Feb. 21, 1711.

It ſeemed to me on the contrary, that it was without having examined the thing cloſe enough, that it has been imagined that the twiſting increaſes the ſtrength of cords, that every thing being well conſidered, we ſhall find perhaps, that far from increaſing, it diminiſhes it; and that this is one of thoſe phyſical problems which cannot be reſolved but by phyſical experiments. Beſides I thought it would be of ſome uſe to mechanicks to endeavour to reſolve them. We might often expoſe the cords that we make uſe of, to be broken, if we ſhould reckon too much upon their ſtrength.

All that they do in making of cords, or in twiſting the threads about one another, is to put them all in a ſtate of contributing ſomething to ſuſtain the force, or the weight that is made to act againſt this cord; and at the ſame time to diſpoſe each thread in ſuch a manner, that it is eaſier to break it, than to make it ſlip, or to diſengage it from thoſe which ſurround it. It is this which gives the facility of making very long cords with very ſhort threads, as we ſee in the cords of hemp, flax, wool, and ſilk, for we may look upon the filaments of ſilk, and wool which they commonly make uſe of, as little cords; each thread being preſſed againſt thoſe that ſurround it, and being twiſted with theſe threads, oppoſes by its friction ſuch a reſiſtance to the force which draws it, that it is more difficult for this force to overcome the reſiſtance of the friction, than to break the the thread.

But does it follow from this diſpoſition of the threads, that the ſum of their forces is ſmaller or greater, than the forces of the cords are? It is not poſſible to decide this by reaſoning alone. We ſee plainly that in twiſting ſeveral threads together, we ſhorten each thread, and that the cord gains in thickneſs, what each loſes in length; if we regard the cord only on that ſide, it is evident that its force is increaſed. For every thing elſe being equal, the thickeſt cords are the ſtrongeſt. If for example we make a cord by twiſting 5 threads about one another, and that the twiſting ſhortens each thread $\frac{1}{5}$, it is evident that the thickneſs of the cord gains $\frac{1}{5}$ by which the length of the threads are diminiſhed; from whence it ſeems evident, that the ſtrength of this muſt be equal to the ſum of the forces that 6 threads would ſuſtain ſeparately.

There is alſo another way, in which the twiſting ſeems to augment the ſtrength of the cord; it is becauſe the weight, which draws the cord, draws each thread obliquely; ſo that one part of this weight is employed to preſs theſe threads one againſt the other; being each leſs drawn according to their length, the cord which they compoſe might be in a ſtate of reſiſting a greater effort, than that which all the threads that compoſe it can ſuſtain, when they are drawn perpendicularly.

Theſe are the favourable ſides on which we can view the twiſting: but on other ſides we ſhall ſee that it weakens the force of the cords, if we would endeavour that in order to a cord's having a force equal to the ſum of the forces of the threads which compoſe it, the weight faſtened to one of its extremities, ſhould act againſt each thread only in proportion to the force of this thread. For if the weakeſt threads are charged as much as the ſtrongeſt, or if ſome threads of equal ſtrength are much more charged than others, they will break, and the weight will fall upon the threads, which before were the leaſt charged. Now the weight that draws a cord, draws each thread that compoſes it, more or leſs, in proportion as this thread is more or leſs ſtretched, and more or leſs thick; and in twiſting theſe threads it is impoſſible to diſpoſe them in ſuch a manner, that the weakeſt ſhould be leſs ſtretched than the others; ſometimes the thickeſt are the weakeſt; each thread therefore does not contribute in proportion to its ſtrength to ſupport the weight; and if for example, in a cord compoſed of 6 threads there are 4 which contribute only half their ſtrength to ſuſtain the weight, the cord muſt be only conſidered as if it was compoſed of 4 threads.

Beſides ſince in twiſting the threads, they are ſtretched; it is plain that the twiſting is equivalent to a weight which would draw each thread, and to a weight greater or leſs according as the tenſion that it produces is greater or leſs; that is, the more this thread is ſtretched, the leſs it is in a ſtate to ſuſtain a weight equal to that which it would ſuſtain naturally. The twiſting alone is ſufficient ſometimes to break the threads, as we find by experiment, when we ſee them too hard twiſted.

The ſame twiſting, which increaſes the ſtrength of the cords in ſome places, diminiſhes it in others. But does the increaſe ſurpaſs the diminution? here geometry has no effect any further than we make arbitrary ſuppoſitions, which conſequently determines nothing: we cannot know whether among theſe ſuppoſitions we have choſen thoſe which are conformable to the effects of nature; we muſt therefore here, as in all other philoſophical doubts, have recourſe to experiments; thoſe under conſideration are ſimple and eaſy to execute. I ſhall relate exactly a part of thoſe that I have made; they will teach us what we are to think of the increaſe of the ſtrength of cords, above that of the ſum of their threads.

Exp. I. I took a bottom of white thread, ſuch as is commonly uſed, and having cut off a great piece of it, I fixed at one end different weights, from 1 pound to 10; this bit of thread ſuſtained 9 pound ½ without breaking, and it broke when I had fixed to it a weight of 10 pound. It was therefore evident that each of the two parts which remained after the diviſion of this thread, could at leaſt bear a weight of 9 ½ pounds, ſince they had already ſuſtained it without breaking. I afterwards doubled the longeſt of theſe ends of thread, and twiſting the two peices together, of which this thread conſiſted, I formed a little cord compoſed of two threads, each of which was able to bear 9 pound ½, conſequently if the twiſting had increaſed the ſtrength of the cord,

above

above the ſum of the ſtrength of the threads which compoſe it, this cord ought to have born above 19 pound; it was very well twiſted without being too much ſo. Nevertheleſs this cord broke when I had ſuſpended a weight of 16 pound to it, and it only ſuſtained 15 pounds ½ without breaking; ſo far from its ſtrength being increaſed by the twiſting, it was diminiſhed about ⅙.

Exp. II. I afterwards fixed a weight of 6 pounds ½, to another thread taken from the ſame bottom, it ſuſtained it without breaking, and broke with 7 pound, I alſo fixed ſeveral weights to 2 other threads the firſt of which reſiſted a weight of 8 pounds, and broke at 8 pounds ½, and the ſecond ſuſtained 8 pounds ½, and broke at 9. I took the longeſt end of each of theſe 3 threads, and twiſted them into a ſmall cord of three threads: the ſum of the forces of theſe three threads was at leaſt capable of ſuſtaining a weight of 23 pounds. The cord nevertheleſs broke when it was charged with 17 pounds ½, the twiſting has therefore conſiderably weakened it.

Exp. III. Having likewiſe taken 4 bits of thread, and knowing by the experiments, that the firſt could ſuſtain 8 pounds ½, and that it broke with 9; that the ſecond could ſuſtain 6 pounds ½, and broke with 7, and that the other two had born 7 pounds, and broke with 7 ½; I made a cord by twiſting theſe 4 threads, I knew by the experiments juſt mentioned, that the ſum of the forces of theſe threads could at leaſt ſuſtain a weight of 29 pounds; I therefore eaſily knew that the ſtrength of this cord was leſs than that of the ſum of the threads, when I ſaw it break after having hung to it a weight of 21 pounds ½.

Exp. IV. To confirm the preceding experiments, I made a new cord as above compoſed of

5 threads, 4 of which had born 7 pounds, and broke with 7 pounds and $\frac{1}{2}$; and the fifth had born 6 pounds, and broke with 6 pounds $\frac{1}{2}$; the ſum of the ſtrength of theſe threads was therefore at leaſt 23 pounds; the cord however broke after having for ſome time ſuſtained a weight of 22 pounds. As I knew by the preceding experiments, and by ſeveral others which I do not think it neceſſary to relate, that the thread which I made uſe of, had in the weakeſt places ſufficient ſtrength to ſuſtain a weight of 6 pounds, and that it was often ſtrong enough to ſuſtain 9 pound; I thought it right to make my calculations, without examining any more the ſtrength of the thread which I uſed, and that when I ſhould find that the ſtrength of the cord ſhould be leſs than that of the ſum of the threads, by conſidering them as not being able each of them to ſuſtain above 6 pounds, that I ſhould not run any riſque of being miſtaken, ſince I had never found the ſtrength of theſe threads leſs, and that I had commonly found them greater. I therefore again made different cords with the ſame thread, becauſe we cannot too often repeat the experiments, before we conclude any thing from them.

Exp. V. I made a cord with 6 threads; it ought at leaſt to have ſuſtained 36 pounds if the ſtrength had been equal to that of the ſum of the threads, and this cord broke with a weight of 31 pounds.

Exp. VI. A cord of 10 threads very well twiſted, which ſhould at leaſt have ſuſtained 60 pounds, if its force had not been leſs than that of the ſum of the threads, broke being charged with 50 pounds.

Exp. VII. Having made a cord by doubling the longeſt of the two ends, which I had left of the preceding cord as it was compoſed of 10 threads; we ſee that I made a cord of 20 threads, which could not carry leſs than 120 pounds, without being much weaker than the ſum of the threads, and leſs than 100 if its ſtrength was not diminiſhed by the laſt twiſting. A weight of 80 pounds broke this cord; its ſtrength was therefore diminiſhed by the laſt twiſting.

Exp. VIII. Another cord made of 28 threads, which would at leaſt have born 168 pounds, if the twiſting had not diminiſhed the ſtrength of the cord, was broken by a weight of 82 pounds; I made ſeveral other experiments, which had the ſame ſucceſs, and which it would be to no purpoſe to relate; However that it may not be imagined that the cords which I made were too much or too little twiſted, and that perhaps the ſame thing does not happen to the cords of thread, or hemp, made by the ropemakers, I made trial of theſe laſt. Among the various experiments that I have tried upon theſe ſorts of cords, I ſhall content myſelf with relating the 2 following, becauſe all thoſe that I made have not ſucceeded differently.

Exp. IX. I took a ſmall hempen cord, very well made by a rope-maker; it was made of three other ſmaller cords, each of which was compoſed of two coarſe threads of hemp. I call thoſe threads which are not made of other ſmaller cords, but are compoſed of divers filaments of hemp or flax. Having fixed a weight of 50 lb. to the cord juſt mentioned, it broke in an inſtant; as it ſeemed to me that this cord ought to have been ſtronger, I ſuſpended afterwards ſeveral weights to the longeſt end that was left; it ſuſtained 72 lb. and broke being charged with 75. To know if the ſum of the forces of the three little cords which compoſed this, was greater than that of this cord, I untwiſted it, and having tried the ſtrength of theſe little cords by different weights, I found that one bore 27 lb. without breaking, the other 33 lb. and the laſt 35 lb. The ſum of the ſtrength of theſe 3 cords was therefore at leaſt equal to that which is required to a weight of 95 lb. yet the cord which they compoſed had firſt broken at 50 lb. and afterward at 75; its ſtrength was therefore much leſs than that of the ſum of the threads.

As to the reſt it muſt be obſerved, that if I had ſought the ſtrength of the 2 threads, which each of the 3 little cords was compoſed of; the ſum of the forces of theſe two threads, would perhaps have been found leſs than that of the little cord which they compoſed; and that by a reaſon particular to cords which are made of filaments, ſhorter than the cords themſelves. Which is, that each of the filaments cannot exerciſe its whole ſtrength, unleſs the reſiſtance of the friction which it muſt overcome to ſlip, does not ſurpaſs the ſtrength which this filament has to ſuſtain a weight. Now it often happens, that the threads are not enough twiſted, becauſe the filaments of hemp, or flax, which compoſe them, cannot ſlip ſo eaſily as not to be broken. But when we make a cord, for example, with two or three of theſe threads, the new twiſting which is given them, adds to the filaments which compoſe them, what they want of friction, and puts them in a ſtate of being broken by a leſs force than is neceſſary to make them ſlide, and when each filament ſhall be more eaſily broken than diſengaged from thoſe which encompaſs it, the ſtrength of the cord will always be leſs than the ſum of the forces of the threads or filaments which compoſe it.

*Exp.*X. Another cord pretty near the ſame thickneſs with the preceding, will alſo ſerve for a new proof. It ſuſtained a weight of 70 lb. and broke, about the middle by the weight of 72; I faſtened a weight of 75 lb. to the longeſt bit that remained, to ſee if the cord had not broken in a place much weaker than the reſt; but it could not ſuſtain the weight of 75 lb. Having ſought ſeparately the ſtrength of the 3 little cords of which it was made, the firſt bore 24 lb. and broke with 28; the ſecond bore 28 lb. and broke with 29; and the third ſuſtained 30 lb. and broke with 31. The ſum of the ſtrength of theſe 3 cords was therefore at leaſt equal to 82, and conſequently greater than that of the broken cord by a weight of 71 lb.

There is no doubt but that the experiments which I made, would have ſucceeded in the ſame manner upon thicker cords; the great number of threads, or of ſmall cords, cannot make any alteration. But the experiments would have been much more difficult to execute, and the preceding are ſufficient. I ſhall however relate one, which I made upon a bit of ſilk, ſuch as is commonly uſed for ſewing; notwithſtanding the ſmallneſs of theſe ſorts of cord, we may compare it to the thickeſt cables, if we only conſider the number of ſingle threads which compoſe it. The threads of this bit of ſilk were exceeding fine; it alſo contained a much greater number of filaments than the bits which I have ſpoken of in the *Examination of the ſilk of ſpiders**. For having ſeparated it with great attention and papatience, I divided it into 832 ſingle threads, whereas I never found but 200 threads in the others. If there was any miſtake in this calculation, it could be only in making the number of threads leſs than it really was, becauſe it might eaſily happen that the extreme fineneſs of theſe threads might make me ſometimes take two for one; but this number cannot be too great, becauſe I never counted one thread without ſeparating it well from the others. I had alſo the precaution to cut it after having counted it for fear I ſhould make a double work of it.

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* Page 41 of this volume.

Theſe 832 threads compoſed two different little cords, which being twiſted about one another, made the bit of ſilk; having ſucceſſively fixed different weights to this bit of ſilk, I found that it commonly ſuſtained 5 lb. for ſome moments, after which it broke; but it was ſeldom ſtrong enough to bear 5lb. ½ and in a great number of experiments it was not above once or twice, that 5 lb. ½ did not make it break. Having afterwards examined the ſtrength of the threads which compoſed this piece of ſilk, I was convinced by ſeveral experiments, that the weakeſt could ſuſtain a dram without breaking; and the ſtrongeſt a dram and ½; we ſee therefore, that if theſe threads are much finer than thoſe which I ſpoke of in the *Examination of the ſilk of ſpiders*, they are alſo much weaker, for thoſe ſuſtained 2 drams ½. Since theſe threads bore at leaſt a dram, and the ſtrongeſt, of which I found a much greater number than of the weakeſt, bore 1 dram and ½, it is evident that I am not too favourable to the ſum of the forces of the thread, when I take 1 dram, 18 grains for the mean force of each thread. And according to this ſuppoſition, the ſum of the forces of the thread which compoſe this piece of ſilk, was 1040 drams; or dividing this ſum by 128 to reduce it into pounds; the ſum of the forces of the threads was 8 lb. 2 ounces. Now we have ſeen above, that the ſilk did not in general ſuſtain above 5 lb. and but ſeldom 5 $\frac{1}{2}$; its force was therefore conſiderably leſs than the ſum of the threads? If we had taken the force of the weakeſt threads, which was a dram, for the true force of each thread, the ſum of the forces would have been 832 drams, that is, 6 lb. $\frac{1}{2}$ and conſequently greater than that of the bit of ſilk.

We may therefore certainly conclude from all theſe experiments, that the force of a twiſted cord is leſs than the ſum of the forces of the threads which compoſe it. But it is not poſſible to determine in what proportion the twiſting diminiſhes it, becauſe this diminution depends upon a great number of irregularities, each of which may be combined in many different manners.

Theſe experiments teach us at leaſt, that when we can employ, in a convenient manner, many little cords, and can ſtretch them equally; theſe little cords would be in a ſtate of producing a greater effect, or of reſiſting a greater effort, than a cable compoſed of all theſe ſmall cords would be.

Laſtly, if we cannot decide what the ſtrength of a cable is, we may decide between what limits it is incloſed, by ſeeking what the force of one of the ſmall cords is which compoſe it; and by examining what is the number of theſe cords; ſince we have ſeen, that the force of the cable is leſs than the ſum of the forces of all theſe cords.

IV. *Remarks on ſome colours, by M.* de la Hire

The deep purple red only appears bright and ſhining when it is expoſed to a ſtrong light; but when we view it in a moderate light, it appears to us very dark, and approaching to black.

We alſo know, that when we view a luminous or very bright body through a dark and rare one, it appears red to us, as when we look at the ſun through a ſmoaked glaſs; and we cannot ſay, that it is the proper colour of this ſmoke which gives it this red, ſince this very ſmoke being mixed with white, makes a colour very much inclinable to blue, which is far from red.

For the explaining this red colour, we muſt have recourſe to what we can imagine of the ſenſation of red, which is only a violent ſhaking of the *retina*, with a certain modification, which is not found in the violent ſhaking of the *retina*, by reflection alone, which only cauſes white; and if the choroïdes, which, according to my ſyſtem, receives the impreſſions of light, to tranſmit them to the *retina*, is very ſenſible, and very thick, it muſt happen that the modified light which gives us the ſenſation of red, meeting this choroides, is intirely abſorbed, and ſhakes the *retina* only as if it was a black body. This is alſo what we obſerve in ſome eyes, which being otherwiſe very good for ſeeing the ſmalleſt objects very diſtinctly, ſee red only as if it was black, and have no idea of what we call red; and for other colours they ſee them perfectly well.

We know alſo, that when we ſee a black body through a white and thin one, it gives us the ſenſation of blue, and there can be no doubt of it, ſince this is the only reaſon why the ſky appears blue to us; for its immenſe depth being intirely deprived of light, can only appear to us, through particles of air, which are enlightened by the ſun, and appear white. This is alſo the reaſon why the black of ſmoke diluted with white, appears blue; for the bodies which appear white, being always a little tranſparent, and being confounded with the black behind them, give a ſenſation of blue.

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* March 28, 1711.

Theſe two explications of red and blue, ſhew us why the veins which are ſeen upon the ſurface of the ſkin, eſpecially if it be very white, appear blue to us, altho' they are filled with a very red blood.

For by what I have here above explained, it is evident that the blood which is of a deep red, being incloſed in the veins, is there in ſome meaſure in obſcurity, and conſequently would appear like black, and this black being viewed through the membrane of the vein, and through the white skin, gives us a ſenſation of blue; which cannot happen to the reſt of the skin which is white, and filled with innumerable particles of blood quite to the cuticle, which muſt appear to us of a white inclined to a vermillion, for theſe particles of blood are very much diſperſed: but if it ſhould happen, by any accident, as by ſome blow, that the blood gathers together in a great quantity under the skin in ſome place, the part preſently appears bluiſh, and we ſay it is bruiſed.

It is alſo without doubt this blue colour of the veins, which has led anatomiſts, who inject wax into the veſſels of the body, to ſyringe blue wax into the veins and red wax into the arteries, to diſtinguiſh them from the veins, and to ſhew a little the different nature of the blood of theſe veſſels, for it is much more vivid, ſpirituous and florid in the arteries than in the veins.

V. *Reflections on ſome new obſervations of F.* Feuillée *made in the* Weſt-Indies, *extracted from a letter written to M.* le Comte de Pontchartrain *from* Lima, Dec. 7, 1709, *by M.* Caſſini, *the ſon*†.

At the beginning of his voyage he was obliged by contrary winds, to ſtop at *Sardinia* and *Malta*, which gave him the opportunity of making ſeveral aſtronomical and phyſical obſervations, the extract of which is related in the *Memoirs of the Academy of* 1708. He alſo made ſeveral obſervations in the *Mediterranean*, for determining the latitude of the places where he had the convenience of obſerving, of which this is an extract.

Height of the pole at Golfo di Palma, *in the iſland of* Sardinia.

	°	′	″
The height of the pole of this gulf which is between the iſland of *St. Antiocho*, and the main land of *Sardinia*, was obſerved to be ——	38	59	24

Height of the pole at Port-Mahon.

	°	′	″
The height of the pole at *Port-Mahon*, which is in the iſland of *Minorca*, was obſerved to be ——	39	53	45

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† Dec. 17, 1710.

Height

Height of the pole at Carthagena.

The height of the pole at *Carthagena*, in *Europe*, was obſerved in the port to be —— —— 37° 36′ 8″

Height of the pole at Almeria.

The height of the pole at *Almeria*, which is in the kingdom of *Granada*, was obſerved to be ——— 36° 50′ 18″

F. *Feuillée* afterwards paſſed the ſtreights of *Gibraltar*, continuing his way toward *America*. He obſerved during the courſe of his navigation, that the ſea-water diminiſhed in its weight, in proportion as he approached the line. He made daily obſervations of it in preſence of the officers of the ſhip, and does not believe that the mixing of the freſh-water has contributed to this alteration, having paſſed the line at a very great diſtance from *Africa* and *America*. He obſerved alſo during his voyage, the variation of the needle, and the place of his courſe where there is no variation.

The firſt place where he arrived in *America*, was *Buenos-Ayres*, where the bad weather would not permit him to make any obſervations of the ſatellites of *Jupiter*.

Obſervations made at Buenos-Ayres *on the* Rio del Plata, *the* 19*th of* Aug. 1708.

The height of the pole was obſerved at *Buenos-Ayres* to be ——— 34° 34′ 44″

This obſervation was confirmed by thoſe of the 20th and 21ſt of *Aug*. which give the ſame height to the pole within a few ſeconds.

The 21ſt of Aug.

F. *Feuillée* obſerved at *Buenos-Ayres* the variation of the needle to be 15° 32′ NE

He obſerved alſo, that the needle of the compaſs dipped at the ſouth point, and made an angle with the true horizon of ——— 6° 20′

Obſervations made at Monte Vidio, *for the height of the pole, the 23d of* Oct.

F. *Feuillée* obſerved at *Monte Vidio*, which is to the ſouth-eaſt from *Buenos-Ayres*, at the outlet of the *Rio del Plata*, the height of the pole to be 34° 52′ 30″

It was obſerved the 24th and 28th, to be within a few ſeconds the ſame.

Obſervations made at the Conception *in the kingdom of* Chili.

F. *Feuillée* determined the height of the pole at the *Conception*, by a great number of obſervations of the meridian height of the ſun, and fixed ſtars, made in *Jan.* and *Feb.* between which, if we take a mean, we ſhall have for the height of the pole at the *Conception* 36° 44′ 30″

Eclipſes of the ſatellites of Jupiter *for the longitude of the* Conception, *the 31ſt. of* Jan. 1709.

At 0h 3′ 23″ in the morning, immerſion of the firſt ſatellite into the ſhadow of *Jupiter*.

5 5 28 at *Paris* by the corrected calculation.

h ′ ″

5 2 5 difference of the meridians between *Paris* and the *Conception.*

The 7th of Feb.

1 55 36 in the morning immerſion of the firſt ſatellite into the ſhadow of *Jupiter.*

6 58 8 at *Paris* by the corrected calculation.

5 2 32 difference of the meridians between *Paris* and the *Conception.*

The 9th of Feb.

11 0 52 at night, the immerſion of the third ſatellite into the ſhadow of *Jupiter.*

16 1 50 at *Paris* by the corrected calculation.

5 0 58 difference of the meridians between *Paris* and the *Conception.*

The 17th of Feb.

0 34 4 in the morning immerſion of the third ſatellite into the ſhadow of *Jupiter.*

5 31 5 at *Paris* by the corrected calculation.

4 57 1 difference of the meridians between *Paris* and the *Conception.*

The 18th of Feb.

1 45 2 in the morning immerſion of the ſecond ſatellite in the ſhadow of *Jupiter.*

At 6h 46' 17" at *Paris* by the corrected calculation.

5 1 15 difference of the meridians between *Paris* and the *Conception*.

Taking the mean between the differences of the meridians determined by the obſervations of the firſt ſatellite of *Jupiter*, which only differ 19 ſeconds from one another, we ſhall have the difference of the meridians between *Paris* and the *Conception* — 5h 2' 14"

This difference being brought into degrees, gives the difference of the longitude between *Paris* and the *Conception* of —— —— 75 33 30

By which the *Conception* is more weſterly than *Paris*.

Obſervations of the variation and inclination of the needle, the 27th of Jan. 1709.

The variation of the needle was obſerved at the *Conception* to be 10° 20' NE

And the inclination of the needle 6 35

Obſervations made at Valparaiſo, *upon the coaſt of the kingdom of* Chili.

F. *Feuillée* determined by the meridian heights of the ſun and fixed ſtars, the height of the pole at *Valparaiſo* to be —— —— 33° 0' 11"

Eclipſe of the firſt ſatellite of Jupiter, *for the longitude of* Valparaiſo, *the* 11*th of* March, 1709.

h ′ ″
At 10 34 13 at night, immerſion of the firſt ſatellite into the ſhadow of *Jupiter.*

15 32 28 at *Paris* by the corrected calculation.

4 58 15 difference of the meridians between *Paris* and *Valparaiſo*, which being brought into degrees, gives the difference of longitude between *Paris* and *Valparaiſo* —— —— ° ′ ″ 74 33 45

By which *Valparaiſo* is more weſterly than *Paris.*

Eclipſe of the ſun, March 11, 1709.

The horizon was full of clouds at the riſing of the ſun, and they could not obſerve it till a little after.

The 11*th of* March.

h ′ ″
At 6 19 46 in the morning, the ſun was eclipſed one digit.

6 27 37 end of the eclipſe.

F. *Feuillée* could only obſerve theſe two phaſes of the eclipſe; and he obſerves, that in this country the ſun is but ſeldom ſeen.

This eclipſe having been obſerved at *Paris*, and ſeveral other parts of *Europe*, we have deſcribed the parallel of *Valparaiſo* in the figure of this eclipſe, and found by the phaſe of a digit, the difference of the meridians between *Paris* and *Valparaiſo* to be h ′ ″ 4 55

And by the end ——— 4 53 50

Theſe differences are leſs than thoſe which are found by the obſervation of the firſt ſatellite of *Jupiter*, to which it is beſt to keep; becauſe of the ſimplicity of the elements which are uſed for the compariſon of theſe obſervations.

Obſervations made at Lima, *the capital of* Peru.

F. *Feuillée* determined by the meridian height of the ſun, the height of the pole at *Lima* to be —— 12 1 15

Whilſt he ſtayed at *Lima* he obſerved geometrically the height of a mountain which he found elevated 143 toiſes, and almoſt 5 feet above the horizon. He took at the foot of the mountain, the height of the barometer, which he found to be 27 inches 5 lines, which is 10 lines ¾ higher than the 26 inches 6 lines ¼, which he found at the top of the mountain.

According to the rule drawn from our obſervations, the difference in the height of the barometer which agrees to the height of 143 toiſes, 5 feet above the horizon of the ſea, ought to be 12 lines 8/9, greater by two lines than what F. *Feuillée* has found, which makes him judge that the condenſation and dilatation of the air in *America*, is very different from that which is obſerved in *Europe*. We ſhall not give a further detail of this obſervation, F. *Feuillée* having a deſign of meaſuring a ſecond time the height of this mountain before his departure.

With regard to the height of the barometer at the ſea-ſhore, F. *Feuillée* found by the obſervation, which he every day made at land, that it is pretty near the ſame as in *Europe*.

During his ſtay at *Lima* they felt there ſeveral earthquakes, and the 7th of *Dec.* in the morning, the day of the date of his letter, they had two ſhocks ſo ſtrong, that had they laſted a little longer, there would not have been any building that could have reſiſted them.

VI. *An extract of ſeveral obſervations made by F.* Feuillée *in the* Weſt-Indies, *by M.* Caſſini *the ſon**.

Since the obſervations that F. *Feuillée* made in his voyage to the *Weſt-Indies*, which we have related to the royal academy of ſciences, he has ſent others to M. *le Comte de Pontchartrain*, which he has continued to make in the *South-ſea.*

Obſervations made at Coquimbo *for the longitude, the 16th of* April, 1710.

	h	′	″	
At	11	57	36	at night, immerſion of the ſecond ſatellite in the ſhadow of *Jupiter.*
	16	51	32	at *Paris* by the corrected calculation.
	4	53	56	difference of the meridians between *Paris* and *Coquimbo.*

The 22*d of* April, 1710.

At	0	6	25	in the morning immerſion of the firſt ſatellite into the ſhadow of *Jupiter.*
	5	1	8	at *Paris* by the corrected calculation.
	4	54	43	difference of the meridians between *Paris* and *Coquimbo*,

Which being brought into degrees, gives the difference of the longitude between *Paris* and *Coquimbo* of 73 40 45

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* July 8, 1711.

Obſervations made at Coquimbo *for the height of the pole, the 17th of* April, 1710.

F. *Feuillée* obſerved at *Coquimbo* the meridian height of the upper edge of the ſun, to be —— 49° 51′ 50″

He continued obſerving it the following days till the 28th of the ſame month.

According to theſe obſervations, having regard to the refraction, the ſemi-diameter of the ſun, and its parallax, we ſhall find the height of the pole at *Coquimbo* to be —— 29° 54′ 30″

Obſervations of the variation and inclination of the needle at Coquimbo.

The variation of the needle was obſerved at *Coquimbo* to be — 8° 32′ NE

And the inclination of the needle 5 25

Obſervations for the height of the pole at Arica, *the 20th of* May.

F. *Feuillé* obſerved at *Arica* the meridian height of the upper edge of the ſun to be —— 51° 49′ 45″

And the 21ſt of *May* —— 51 37 34

By theſe obſervations we find the height of the ſouth-pole at *Arica* to be 18 28 0

Obſervations made at Ylo *for the longitude, the 24th of* July, 1710.

At 9h 24′ 57″ at night, emerſion of the firſt ſatellite out of the ſhadow of *Jupiter*.

h ' "
At 14 19 11 at *Paris* by the corrected calculation.
4 54 14 difference of the meridians between *Paris* and *Ylo*,

Which being reduced into degrees gives the difference of longitude between *Paris* and *Ylo* of —— ° ' " 73 33 30

By which *Ylo* is more weſterly than *Paris*.

Obſervations for the height of the pole at Ylo, *the 5th of* June.

F. Feuillée, obſerved at *Ylo* the meridian height of the upper edge of the ſun to be — — — — — ° ' " 50 5 30

He continued obſerving it the following days 'till the 25th of *July*.

According to theſe obſervations we find the height of the pole at *Ylo* to be ° ' " 17 36 30

Obſervations of the variation and inclination of the needle at Ylo.

The variation of the needle was obſerved at *Ylo* to be —— ° ' " 6 38 0 NE
and the inclination of the needle 3 45 0

Theſe obſervations, added to thoſe which we have already related, determine more particularly the ſituation of the weſt coaſt of *South America*, which 'till now has been but little known.

VII. *Experiments on the thermometer, by* M. de la Hire *the ſon**.

A canon of *Chartres* one of my friends, and an excellent naturaliſt, wrote to me in *February* 1709, that having ſeen the papers that *M. Nuguet* had publiſhed, where he gave the conſtruction of a new thermometer, which as he pretended was free from the faults of others, and that having alſo ſeen the reflections that I have given upon this thermometer, printed in the *memoirs of the Academy* of 1706, he had a mind to examine whether this new thought, ſo oppoſite to that of M. *Amontons*, was founded upon any certain principle.

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* June 13th, 1711.

He took for his experiments a thermometer which he had cauſed to be made at *Chartres*, from one of thoſe of M. *Amontons*, the ball of which was 13 lines outſide diameter, and the tube 3 feet 2 inches long, to $\frac{2}{3}$ of a line of inſide diameter.

The 7th of *December* 1708, he put this thermometer into water, which he let freeze, without taking notice at what height the ſpirit of wine was in the tube, and when the water was perfectly frozen, the ſpirit of wine was at 11 inches 7 lines above the ball; he afterwards melted the ice from the thermometer, by holding it to the fire, and believed that the ſpirit of wine could not deſcend lower in this thermometer; not imagining that there could be a greater cold than that of water very hard frozen.

He afterwards expoſed this thermometer to the cold of the following days, and found that the ſpirit of wine fell in the tube to 1 inch lower than it had fallen being in water very hard frozen, that is, 1 inch below the 11 inches 7 lines; he believed that the cauſe of this effect proceeded from this, that the water was not perfectly frozen in its whole maſs, for this reaſon he repeated the preceding experiment.

January the 8th 1709, the cold being very great, at 8 in the morning he put the ſame thermometer, of which the ſpirit of wine was at 9 inches 8 lines above the ball, into water which was frozen in a very little time, and examining very attentively what ſhould happen to the ſpirit of wine in the tube, he ſaw that in leſs than half a quarter of an hour, the ſpirit of wine roſe 2 inches ½ a line above the 9 inches 8 lines. An hour after, it was alſo riſen ½ a line more, at noon another ½ line, and at half an hour after 9 at night, it was riſen 2 inches 4 lines ⅓ above the nine inches 8 lines. He left this thermometer all night to the air and in the ice, the cold being very great, ſo that the ice ſwelled and raiſed itſelf above the edge of the veſſel, and the next day in the morning about the ſun's riſing, he found that the ſpirit of wine was riſen in the tube more than 2 feet above the ball, and that it was interrupted with many bubbles of air very much extended. He here finiſhed his experiment, and wanting to diſengage the thermometer from the ice, he broke it.

This experiment did nothing but puzzle my friend, for he ſaw the contrary happen to what he expected, and to what had happened in the firſt experiment, ſince inſtead of ſeeing the ſpirit of wine ſunk in the tube after the thermometer was in the ice, he ſaw it riſe continually to the height of above 2 feet, and interrupted with many bubbles of air, which made him think that the ice had perhaps made the ſpirit of wine ferment, from whence he concluded that *M. Nuguet*, who made uſe of the cold of water in which he put ice to conſtruct his thermometer, would have found it difficult always to meet with the ſame degree of cold.

I anſwered my friend ſome days after, and acquainted him that the experiment had ſurprized me, and that I did not believe the great cold had cauſed a fermentation in the ſpirit of wine, but I was perſuaded that what made it riſe 2 inches $\frac{1}{2}$ a line, during the firſt half quarter of an hour, that the thermometer was in the water, was, that when he had put it in, the ſpirit of wine, which was extremely condenſed by the great cold, becauſe it was expoſed to the air, and the water was not, had in a manner thawed, that is, the particles of cold which were within, had gone out, and were joined to thoſe of the water, which was not ſo cold as the ſpirit of wine, ſince it was not frozen, as it happens to fruits that are frozen, when they are put into water that is near freezing.

As to the elevation of the other 4 lines, which the ſpirit of wine roſe in the tube, during the reſt of the day, it might very well come from this, that the water being afterwards frozen very hard, it had according to cuſtom increaſed its bulk, and made an effort againſt the ſides of the veſſel, and againſt the ball of the thermometer, of which it had by this means diminiſhed the bulk.

After having given him a reaſon for the elevation during the day, it remained to explain how the ſpirit of wine had been able to riſe to ſo great a height in the night, and from whence the bubbles of air came, which were found mixed with it.

I could not find a more probable explication, not knowing how to admit the fermentation, unleſs it was that the water having continued to freeze more and more, had conſiderably increaſed its bulk, ſo that the ſides of the veſſel not being able to give way, the whole effort was united againſt the ball of the thermometer, which not having ſtrength enough to reſiſt it, for want perhaps of not having been perfectly ſpherical, or of the ſame thickneſs in its whole extent, was broken; and then the preſſure of the ice, joined with many bubbles of air which eſcaped from it one after another, and were entered into the ſpirit of wine, had obliged it to riſe in the tube to ſo great an height, being interrupted with bubbles of air.

Theſe were the reaſons I then made uſe of, to explain to my friend the experiments he had communicated to me; however, as I was afraid of being miſtaken in my conjectures, I reſolved the next winter to make his experiment; but having only one cold day in 1710, I could not execute what I had projected, and was obliged to defer it till this year 1711, in which we have had ſome days cold enough do it.

The 2d of *February* at 11 in the morning the thermometer, which always is in the eaſt tower of the obſervatory, and with which we make our experiments, being at 24 ½ parts, I expoſed upon the window of this tower, which looks to the north, an iron mortar full of water, in the middle of which I had ſuſpended the ball of a thermometer which I had carried into the caves of the obſervatory, to have a fixed point from whence I might meaſure the fallings and riſings of the ſpirit of wine in the tube, becauſe we know that the temperature of the air never changes in theſe caves, and that it is taken for a mean ſtate.

After the thermometer had been a quarter of an hour in the water, upon which there began to be formed a cruſt of ice, I found that the ſpirit of wine was fallen in the tube 3 inches 1 line below the mean ſtate, I continued obſerving

ſerving it every quarter of an hour, 'till 5 in the afternoon, to ſee the alterations which might happen to it, but I did not obſerve any during the whole time, and the ſpirit of wine always remained at the ſame place, 'tho the water was continually more and more frozen, and then ſeemed to me entirely ſo through the maſs; the thermometer with which we made our experiments had not changed ſince the morning, and had always remained at 24 ½ parts.

I left my thermometer in experiment, in the ſame place, all night, between the ſecond and third, and the 3d at 8 in the morning, our common thermometer being at 21 parts, that is at 3½ parts lower than the preceding day, I found that which was in experiment in the ice, the ſurface of which was ſplit in many places, probably by the ſtrength of the cold, was ſunk 5 inches 10 lines ⅔ below the mean ſtate, and conſequently 2 inches 9 lines ⅔ lower than the preceding day.

The ſame day at 11 in the morning, I put the thermometer, that was in experiment, near our common thermometer, which was at 24 ½ parts, as the preceding day; and after having left it there ſome time, I found that which was in experiment riſen again to 13 lines above what it was at 8 in the morning; ever ſince I have always left them together to compare them.

The 4th, at three quarters after ſeven in the morning, the common thermometer was 23 ½ parts, and that which was in experiment was 3 inches 8 lines below the mean ſtate.

The 5th, at half an hour after ſeven in the morning, the common thermometer was at 26 ½ parts, and that which was in experiment was 3 inches 8 lines below the mean ſtate.

The 6th, at half an hour after ſeven in the morning, the common thermometer was at 25 ½ parts, and that which was in experiment was 3 inches 11 lines below the mean ſtate.

The 7th, at half an hour after ſeven in the morning, the common thermometer was at 23 ½ parts, and that in experiment was 4 inches 3 lines below the mean ſtate.

The 8th, at three quarters after ſeven in the morning, the common thermometer was at 23 parts, and that in experiment, was at 4 inches 3 lines ½ below the mean ſtate.

The 9th, at three quarters after ſeven in the morning, the common thermometer was at 30 parts, and that which was in experiment was at 3 inches 1 line ½ below the mean ſtate.

The 10th, at noon, the common thermometer was at 40 parts and ½, and that which was in experiment was at 1 inch 9 lines ¼ below the mean ſtate, and was plunged in the water which was come from the ice in the mortar, and was melted during the preceding night.

In comparing theſe experiments together, we ſee that the thermometer which was in the ice has pretty well followed that which was not, when the cold became greater; but when it diminiſhed, that which was in the ice did not follow it ſo well, and it could not riſe ſo eaſily as the other, becauſe of the cold of the ice, which ſurrounded it.

I was ſurprized the 2d of *February*, when I put my thermometer into experiment, to ſee that a quarter of an hour after it was put there, during which time it had ſunk, becauſe before that time it had been in a place leſs cold than the open air, it did not change for ſix hours, altho' the water was entirely frozen, but the experiments which I made afterwards ſhewed me the reaſon of it, by letting me ſee that the degree of cold of the air, which did not change in theſe ſix hours, was greater than was neceſſary to freeze the water, and ſo the thermometer which was within, could not receive any impreſſion from it; yet it happened quite the contrary to that of my friend, for during the firſt quarter of an hour, it roſe inſtead of falling, from whence it muſt be concluded, that the water was leſs cold than the ſpirit of wine of the thermometer, which was expoſed to the air, and it really was ſo, for it froze in a very little time; but it ought to have ſunk afterwards, ſince the cold paſſed through the ice, which did not happen, by the reaſons which are related at the beginning of this memoir.

As to the great height where he found the ſpirit of wine, interrupted by great bubbles of air in the tube, the next morning, it does not appear to me that this effect could proceed from any other cauſe than the breaking of the ball, as it is here above obſerved, ſince the temperature of the air had hardly changed from that day to the next.

It does not ſeem to me, that it can be ſaid, that the cold of ice is always the ſame cold, ſince we have ſeen by the experiments juſt mentioned, that the greater or leſs cold of the air, is very ſuddenly felt upon the ball of the thermometer which is incloſed in the ice, and if the ſpirit of wine is ſuſceptible of alteration through the ſolid ice, what will it not be when it ſhall be broken, or put into water.

VIII.

VIII. *New experiments on the dilatation of the air, made by M.* Scheuchzer, *upon the mountains of* Switzerland, *with reflections thereon, by M.* Maraldi *; *tranſlated by Mr.* Chambers.

M. *Scheuchzer* having ſent ſeveral experiments on the dilatation of the air, made on the mountains of *Switzerland*, in *September* 1710, to the *Abbé Bignon*, who deſired M. *Scheuchzer* to make ſome further obſervations, to learn whether the dilatation of the air at great heights follow the ſame proportion, as near the ſurface of the earth.

Accordingly with a tube 33 inches long, and 2 lines in diameter, he obſerved the height of the mercury *in vacuo*, at ſeven different ſtations, making the common obſervations at each ſtation, of the dilatation of the air, by leaving firſt 3 inches of natural air in the tube, then 6 inches, then 9, and ſo ſucceſſively to 30. And he meaſured the height at which the mercury ſtood in the tube, after the dilatation, as well as the ſpace poſſeſſed by the dilated air, after the invertion in the loweſt of theſe ſtations, the mercury ſtood *in vacuo* at 26 inches, 7 lines $\frac{1}{2}$, and in the higheſt at 21 inches 6 lines, ſo that the difference between the height of mercury *in vacuo*, was 5 inches; now to find whether the common rule of the dilatation of the air among us, be conformable to the obſervations of M. *Scheuchzer*, I calculated the ſpace which the air was to poſſeſs in the tube after dilatation, according to this rule, and compared them with the obſervations themſelves.

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* March 27, 1711.

By this compariſon it appears that the *calculus*, only agrees with obſervation in the dilatations anſwering to the 3 firſt inches of natural air, in the reſt the dilatation obſerved is leſs than that of the rule, as far as the 18th inch of natural air, where they agree with each other within one or two lines; but from the 18th to the 30th inch of natural air, the dilatation obſerved is always greater than that calculated, contrary to what obtains in the firſt 18 inches, the greateſt exceſs of the *calculus* above obſervation was at the 9th and 10 inch, and was at 8 or 9 lines, and the greateſt deficiency of the *calculus* with regard to obſervation was 10 or 11 lines, at the 24th and 27th inch of natural air:—Which ſhew that at great heights the air does not dilate at the ſame rate as it does near the ſurface of the earth, and conſequently that the rule is not general for the whole compaſs of the air in any climate.

In a former memoir for the year 1709, we have alſo ſhewn that this rule does not hold even in air, much at the ſame height, but in climates very different from our own, as that of *Mallacca* in the *Eaſt-Indies*, here by other obſervations like thoſe of M. *Scheuchzer*'s three inches of natural air, after their dilatation poſſeſs'd a ſpace of 7 inches 5 lines in the tube, whereas by the rule it ſhould have poſſeſs'd 9 inches 6 lines ½, the difference therefore between the obſervation and rule is 2 inches 1 line.———So 6 inches of natural air after dilatation poſſeſs'd 10 inches 9 lines; whereas by the rule the dilatation ſhould have been 13 inches 3 lines, the difference is 2 inches 5 lines, by which the dilatation of the rule comes ſhort of that found by obſervation, and the like is found in ſeveral obſervations made at

Malacca, and calculated in the memoir above mentioned, which ſhews that in the air of *Malacca* the dilatation is much greater than in ours, and even than what is found in M. *Scheuchzer*'s obſervations.

There is ſome conformity however between the obſervations of *Malacca* and thoſe of *Zurick*; at *Malacca*, where the dilatation of the air is different from that of *Paris*, the variation of the barometer is leſs than at *Paris*, and the ſame holds with the obſervations of *Zurick*, where the dilatation of the air is different from what it is at *Paris*, and the variation of the barometer leſs than what is found either at *Genoa* or at *Paris*; 'tis true at *Malacca* the dilatation is different from what it is at *Zurick*, the dilatation obſerved at the former place being always leſs than what ariſes from the *Calculus*, whereas by M. *Scheuchzer*'s obſervations, the dilatation obſerved proves leſs than the calculated one to a certain term, after which it is greater———'Tis obſervable that at the loweſt place where M. *Scheuchzer* made his obſervations, the dilatation of the air proceeds differently from what it does at *Paris*, though the difference in the height of the mercury, at thoſe two places, be only 2 inches, and yet the dilatation proceeds alike at the ſeveral ſtations, obſerved by M. *Scheuchzer*, tho' the difference between the higheſt aud loweſt place anſwered to above 5 inches of mercury, whence it may be inferr'd, that near the ſurface of the earth, the air varies even in the ſame climate, but that 'tis more uniform at a great height therefrom.

M. *Scheuchzer* made ſome experiments in an iron mine, where the air was very hot, on account of the fire kept therein to melt the oar, and found the height of the mercury in *vacuo*, as well as the dilatation of the air, the ſame as they were out of the mine in the open air, which agrees with the experiments related in the memoirs of the year 1709, whereby it appears that a great heat, as that of boiling water, makes no ſenſible alteration in the dilatation of the air.

IX. *Obſervations of ſome eclipſes of the planets and fixed ſtars by the moon, made in ſeveral places, compared together to determine the differences of the meridians, by M.* Caſſini *the ſon*[*].

The obſervations of the eclipſes of the ſtars by the moon, made in ſeveral places, being very proper for determining the geographical longitudes of thoſe places; we have thought it neceſſary to compare many of thoſe which have been hitherto made, to draw this advantage from them.

Among theſe obſervations there are ſeveral which are related in the *Journaux des Scavans*, in the *Philoſophical Tranſactions of the Royal Society of London*, and in the *Leipſick Acts*, from which we have extracted thoſe which have been made at the ſame time in ſeveral places.

Obſervations of the eclipſe of the Pleiades *by the* moon, *made at* Paris, *and at* Dantzick, *the* 23*d of* Aug. 1701.

At $13^{h}\ 31'\ 23''$ at *Paris* the bright ſtar of the *Pleiades* of the third magnitude called *Alcyone* by *Riccioli*, enters into the bright part of the moon.

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[*] Jan. 24, 1711.

h ′ ″
15 0 0 at *Dantzick*, *Alcyone* enters into the bright part of the moon.
16 6 55 at *Dantzick*, emerſion from the dark part.

To find by the means of this obſervation the difference of the meridians between *Paris* and *Dantzick*; they firſt determined the paſſage of this ſtar over the meridian which happened at *Paris* at ——— h ′ ″ 17 15 34
Its right aſcenſion —— 52° 4 30
And its declination —— 23° 3 41

They have alſo calculated the right aſcenſion and the declination of the moon ſome hours before or after its conjunction, its diameter, and horizontal parallax.

By this means they have placed in a figure which repreſents the projection of the earth in the orb of the moon, the parallels of *Paris* and *Dantzick*, and the line that the moon deſcribed in paſſing by this projection.

According to this figure, the immerſion of this ſtar ought to have happened at *Dantzick* at —— h ′ ″ 13 56 0
Which gives the difference of the meridians between *Paris* and this city 1 4 0

By which *Dantzick* is more eaſterly, becauſe the hour obſerved exceeds that which is marked upon the path of the moon prepared for the meridian of *Paris*.

The ſame day

h ′ ″
12 55 25 at *Paris*, *Merope* enters into the bright part of the moon.

h ' "
14 24 30 at *Dantzick* immerſion of *Merope* into the bright part of the moon.
15 15 20 at *Dantzick* emerſion out of the dark part.

	h ' "
The paſſage of this ſtar over the meridian happened at	17 14 26
its right aſcenſion was	51°47 25
and its northern declination	22°51 55
By the immerſion of this ſtar into the bright part of the moon, we ſhall have the difference of the meridians between *Paris* and *Dantzick* to be	1^{h} 4 34

The ſame Day,

h ' "
12 21 20 at *Paris* immerſion of *Electra*, into the bright part of the moon.
13 40 0 at *Dantzick*, immerſion of *Electra* into the bright part of the moon.
14 50 0 at *Dantzick* emerſion out of the dark part.

	h ' "
The paſſage of this ſtar over the meridian happened at	17 12 56
its right aſcenſion was	51°25 6
and its northern declination	22°51 55
By the immerſion of this ſtar into the bright part of the moon, we ſhall have the difference of the meridians between *Paris* and *Dantzick*.	h 1 2 55

Eclipſe of Mars *by the Moon, obſerved at* Dantzick, Oxford, *and* Greenwich, *the* 31*ſt of* Auguſt, 1676.

h ' "
13 35 42 at *Dantzick* immerſion of *Mars* into the bright part of the moon.
14 46 29 emerſion out of the dark part.

h	′	″	
12	10	42	at *Oxford* immerſion of *Mars*, into the bright part of the moon.
13	10	41	emerſion out of the dark part.
12	14	58	at *Greenwich* immerſion of *Mars* into the bright part of the moon.
13	10	51	emerſion out of the dark part.

	h	′	″
There is an error in the hour of this obſervation, for inſtead of —	13	10	51
We muſt read —	13	15	51
Which we ſee by comparing the hour obſerved with the corrected hour, the difference between theſe hours which in all the obſervations is	0	4	55
In this being only —	0	0	5

Having calculated the right aſcenſion, and the declination of *Mars*, and of the moon, at the time of its conjunction, and ſome hours before or after, they deſcribed the path of the moon, in a figure which repreſents the projection of the earth in the orb of the moon, and they have there determined the north pole, and the parallels of *Dantzick*, *Oxford*, and *Greenwich*.

	h	′	″
By the immerſion of *Mars* obſerved at *Dantzick*, and at *Greenwich*, they find the difference of the meridians between theſe places —	1	15	12
And by the emerſion —	1	15	0
By the immerſion of *Mars*, obſerved at *Dantzick* and *Oxford*, they find the difference of the meridians between theſe two cities —	1	20	23
And by the emerſion —	1	20	20

This obſervation is related in the *Journal des Scavans* of the 18th of *Jan.* 1677, where it is remarked that Dr. *Halley* having carefully conſidered the parallaxes of the moon in the obſervations of this eclipſe made at *Oxford*, at *Dantzick*, and at *Greenwich*, he found by the immerſion of *Mars*

	h	′	″
The difference of the meridians between *Greenwich* and *Dantzick* to be	1	14	50
And between *Greenwich* and *Oxford*		4	59
And by the emerſion of the ſame planet the firſt of theſe differences is found to be ——	1	14	41
And the laſt ——		4	59

Theſe differences of the meridians that Dr. *Halley* has probably found by the old method, which is by calculating the parallax of the moon at ſeveral heights, agree within a few ſeconds with thoſe that I have found by the method of the projection of the earth in the orb of the moon.

	h	′	″
Suppoſing the difference of the meridians between the obſervatory of *Paris* and that of *Greenwich* to be		9	10
As we have determined by the obſervations of the *Satellites of Jupiter*, we ſhall have the difference of the meridians between *Paris* and *Dantzick*	1	5	55

A little greater than that which is determined by the obſervation of the *Pleiades* made at *Paris*, and at *Dantzick*.

Eclipſe of Jupiter *by the moon obſerved at* Paris, London, Greenwich, Nuremberg, Leipſick, *and* Avignon, April 10, 1686.

h	′	″	
9	40	21	at *Paris Jupiter* touches the moon.
9	41	20	he quite diſappeared in the inequallities of the moon's limb.
10	30	2	the preceding ſatellite emerges.

h	′	″	
10	40	24	the firſt limb of *Jupiter* begins to emerge out of the dark ſide.
10	40	56	the centre of *Jupiter* emerges out of the moon.
10	41	36	*Jupiter* is intirely emerged.
10	42	49	the neareſt ſatellite to *Jupiter* emerges it, is $\frac{35}{60}$ diſtant from *Jupiter*.
10	45	1	the next ſatellite emerges, it is diſtant from *Jupiter* a little leſs than two of his diameters.
10	50	40	the laſt ſatellite emerges.
9	33	0	at *London*, immerſion of the centre doubtful, becauſe the limb of the moon was not well terminated.
10	30	0	beginning of the emerſion.
10	31	20	*Jupiter* is intirely emerged.
9	32	30	at *Greenwich* the limb of *Jupiter* touches the moon.
9	33	42	*Jupiter* intirely hid.
10	30	30	A little part of *Jupiter* emerged.
10	31	36	*Jupiter* intirely emerged.
10	19	56	at *Nuremberg* by M. *Zimmermam* *Jupiter* touches.
10	20	47	*Jupiter* intirely hid.
11	22	51	*Jupiter* intirely emerged.
11	25	43	the ſatellite which is in the middle of the three emerges.
11	31	6	the third ſatellite emerges.
10	20	50	at *Nuremberg* by M. *Wurtzelburg* *Jupiter* touches.
10	22	0	*Jupiter* half eclipſed.
10	22	30	*Jupiter* wholly hid.
11	19	40	*Jupiter* began to emerge.
11	21	20	*Jupiter* wholly emerged.

	h	′	″	
At	10	30	33	at *Leipſick*, the limb of *Jupiter* touches the moon.
		31	4	immerſion of the centre.
		31	33	*Jupiter* intirely hid.
	11	35	0	*Jupiter* entirely emerged. [*There is here an error of the preſs.*]
	11	7	9	at *Dantzick*, the limb of *Jupiter* touches the moon.
		7	54	immerſion of the centre.
		8	39	*Jupiter* intirely hid.
	11	49	15	beginning of the emerſion.
		50	0	emerſion from the centre.
	11	50	45	total emerſion of *Jupiter*.
	9	42	13	at *Avignon*, immerſion of the centre of *Jupiter*.
	10	45	26	emerſion of the centre.

	′	″
By the firſt obſervation made at *Paris*, and at *Greenwich*, when *Jupiter* touched the moon, they find the difference of the meridians between theſe two places	9	20
By the total immerſion ——	9	33
When a part of *Jupiter* was emerged	9	20
When *Jupiter* was entirely immerged	9	34
By the obſervation made at *Paris*, and at *London*, they find the difference of the meridians between theſe two cities, when *Jupiter* began to emerge ——	9	50
When he was intirely emerged	10	0
By the obſervation made at *Nuremberg* by M. *Wurtzelbaurg*, they find the difference of the meridian between *Paris* and that town when *Jupiter* touched the moon	35	30
By the central immerſion ——	35	0
When *Jupiter* was intirely hid	34	45

	′	″
When *Jupiter* began to emerge	34	55
When he was entirely emerged	35	0

M. *Zimmerman*'s obſervation gives a difference of ſome ſeconds which probably proceeds from the manner of regulating the clock.

By the obſervation made at *Leipſick*, they find the difference of the meridians between *Paris* and that town, when *Jupiter* touched the moon —	40	10
By the immerſion of the centre	40	0
When *Jupiter* was intirely hid.	39	50

	h		
By the obſervation made at *Dantzick*, we find the difference of the meridians between *Paris* and that town, when the limb of *Jupiter* touched the moon ———	1	5	4
By the immerſion of the centre of *Jupiter* —— ——	1	4	44
When *Jupiter* was intirely hid	1	4	39
When *Jupiter begun* to go out of the moon —— ——	1	4	55
By the emerſion of the centre	1	4	40
By the total emerſion ——	1	4	15

As for the obſervation at *Avignon*, it would be uſeleſs to compare it with that which has been made at *Paris*, there being probably ſome error in the time marked by the clock, ſince the difference between the hour of the immerſion from the centre of *Jupiter* into the moon; and that which was obſerved at *Paris*, is only two minutes.

	h	′	″	
Taking a mean between the differences of the meridians which reſult from theſe obſervations, we ſhall have the difference of the meridians between *Paris* and *London* —		9	55	to the weſt.
Between *Paris* and the obſervatory at *Greenwich*		9	25	—
Between *Paris* and *Nuremberg* — —		35	2	to the eaſt.
Between *Paris* and *Leipſick* — —		40	0	—
Between *Paris* and *Dantzick* —	1	4	43	—

Eclipſe of Jupiter *by the moon, obſerved at* Avignon, London, *and* Totteridge, May *the 7th*, 1686.

h ′ ″

15 37 23 at *Avignon*, immerſion of the centre of *Jupiter* into the moon.

16 28 24 emerſion of the centre.

15 3 30 at *Totteridge*, beginning of the immerſion of *Jupiter* into the moon.

The latitude of *Totteridge* is 51° 39′

And according to Dr. *Halley*, this place is 25 ſeconds to the weſt of *London*, and is 9 miles diſtant from it.

h ′ ″

15 49 0 at *London*, total emerſion of *Jupiter*.

Although the obſervation at *Avignon* was made when the centre of *Jupiter* entered into the moon, and when it went out, we may however compare it with thoſe which were made at *Totteridge* and *London*, at the beginning of the immerſion, and at the end of the emerſion, by the means of the diameter of *Jupiter* which ′ ″
was then ——— 0 50

By the firſt obſervation we find the difference of the meridians between *Avignon* and *Totteridge* ——— 20 20

By the emerſion of *Jupiter* obſerved at *London*, we have the difference of the meridians between *Avignon* and *London* 19 20

If we ſubſtract from the difference of the meridians that we have juſt found between *Avignon* and *Totteridge*, 25 ſeconds, by which *Totteridge* is more weſterly than *London*, we ſhall have the difference of the meridian between *Avignon* and *London* — — — 19 55

Taking a mean between the differences which reſult from theſe obſervations, we ſhall have the difference of the meridians between *Avignon* and *London* — 19 40

From which if we ſubſtract the difference between *London* and *Paris*, which we have found by the obſervations of the ſatellites of *Jupiter*, to be — 9 40

We

	′	″
We ſhall have the difference of the meridians between *Paris* and *Avignon*, by which *Avignon* is more to the eaſt	10	0
This difference agrees with that which we have found by the triangles of the meridian	10	7

A
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AN ABRIDGMENT OF THE PHILOSOPHICAL DISCOVERIES and OBSERVATIONS in the HISTORY of the ROYAL ACADEMY of SCIENCES at *Paris*, for the Year 1712.

I. *On Bees.*

THO' the reputation of bees is very ancient, and well eſtabliſhed, yet they have not been thought ſo wonderful as they really are; and we may ſay of them, as we ſometimes ſay of perſons of merit, that they improve upon being acquainted with them. M. *Maraldi*, who has obſerved them for many years, with attention, and diligence, has given a very advantageous and circumſtantial teſtimony concerning them; which we ſhall reduce to the points of greateſt importance, and the moſt eaſy to be underſtood.

The bee gathers both wax and honey from flowers, but not with the ſame organs. As the honey is a liquid ſubſtance, which exudes from the flowers by tranſpiration, the bee ſucks it with a tube from the bottom of the empalements, and attacks only thoſe where the empalement is not too deep for the length of its tube, when it is at full ſtretch; for it is bent double when the bee is not gathering honey. The liquor ſucked up by this tube, is drawn into a little bladder, tranſparent enough to ſhew the colour quite thro'. Part of it ſerves for the nouriſhment of the animal, and is diſtributed into its veſſels. We ſhall ſay what becomes of the other in its proper place. As for the wax, which is the duſt of the ſummits of the flowers, the bees take it with their fore-feet, and put it into a little cavity, which they have in their hinder-feet. They often ſqueeze and preſs it with their feet, not only that they may have room for more, but alſo to reduce it into a fitter form for being carried away. Sometimes they roll themſelves upon the flowers, when they are wet, to carry off little particles of wax, which will ſtick to the hairs that cover their whole body, and ſo load themſelves on all ſides.

When the bee is returned to the hive with its harveſt, it either unloads itſelf in an inſtant; or if it is not able, is ſure to be aſſiſted by others.

The deſign of gathering the wax, is to form that wonderful ſtructure called a comb: it is for this, that the bees have always been admired; and are ſtill more admirable than has been imagined. The hexagonal figure which they give to the cells of their comb, would be worthy of the beſt geometricians, who know that any number of theſe figures fills a ſpace without leaving any void; and that this figure alſo, which has that in common with the ſquare and equilateral triangle, has certainly the advantage of containing a greater ſpace in the ſame circumference. And beſides, of all the geometrical ways of working, they have choſen that which is at the ſame time the moſt ſimple, and the moſt convenient for them; and conſequently they have made a very ingenious choice.

Though there ſeems to be nothing in a hive, but a continual and irregular agitation of ſeveral thouſands of inſects, flying about at random, yet there is in reality a great order; but it muſt be ſtudied with care. The labours are diſtributed as among the beavers. Some bees bring wax between their two jaws, and perhaps they pour out ſome liquor upon it, which dilutes and ſoftens it; ſometimes the ſame bees raiſe from this wax the little walls of the hexagonal cells; ſometimes others have this office; but, in ſhort, thoſe which raiſe the cells are not thoſe which poliſh the work; they are ſucceeded by others which are commiſſioned to make the angles more exact, to ſmooth and poliſh the ſurface, and to give the laſt hand to every thing. And as this cannot be done without cutting off ſome particles of wax, and as the bees are extremly frugal, ſome take care to carry away theſe particles, which we may be ſure will not be loſt. M. *Maraldi* has obſerved, that the bees which raiſe the walls, work leſs time than thoſe which poliſh them, as if the labour of poliſhing was the leaſt fatiguing.

Their diligence is extreme. A comb of a foot in length, and 6 inches in breadth, containing near 4000 cells, is finiſhed in a day. For this, however they muſt have all circumſtances favourable.

They faſten one comb to the top of the hive, from whence they work downwards, provided the top is not a moveable lid; for if it is, they will perceive it, and faſten their comb elſewhere. It is not properly wax that they faſten it withal; they are very ſparing of that, and make uſe of a very coarſe glue.

As the combs are planes perpendicular to the baſe of the hive, which I ſuppoſe to be circular, if there was one of them, of which the bottom was a diameter, or an intire chord of this baſe, it would cut the hive into two parts, which could have no communication together. The bees foreſee this inconvenience, by not making their combs of ſo great an extent, and leaving between two neighbouring combs, which are almoſt in the ſame plane, a ſpace by which two bees may paſs abreaſt. They alſo leave ſome openings in each comb, that they may not be obliged to go ſo far about. Here is a city built with a great deal of skill.

The cells of the combs are intended for two uſes. 1. They are their magazines; they reſerve in them the honey, which is to be their nouriſhment in winter. For of that which they take upon the flowers, and which enters into the veſicle that we have ſpoken of, there is but a ſmall part which ſerves for their actual nouriſhment, they throw out the reſt when they return to their hive, and make proviſion of it. Beſides, they keep in the cells already made, the wax which is to be uſed to make others; or to ſerve for any other purpoſe.

2. The cells are the cradles for the young ones. But whence do theſe young ones come? It is one of the greateſt difficulties relating to this ſubject to make the diſcovery.

The fabulous traveller who ſpeaks of a nation, where there is no diſtinction of ſexes, and where he could not diſcover how generation was performed, might have taken this thought from the bees; and *Virgil* was not in the wrong to praiſe their chaſtity, or even to believe the fable of the bull, for want of a better. In a whole hive compoſed of 8 or 10,000 bees, there is but one perhaps which breeds. It is longer, and of a more lively colour than the reſt; it has a grave and a ſolemn deportment, and is what they commonly call *the king*. There are ſometimes 2 of theſe, or at moſt 3 in a hive, which has cauſed a ſuſpicion, that the priviledge of generation belongs only to one, for it appears from M. *Maraldi*'s obſervations, that it belongs only to this royal ſpecies. The whole body of the people is condemned to barrenneſs.

Moſt commonly the king breeds his young in ſuch parts of the hive where he cannot be obſerved; but when by good fortune he makes choice of others more expoſed to view, it is ſtill generally very difficult to ſee him, becauſe the bees draw a curtain before him. This curtain is their ownſelves hung from top to bottom, and hooked to each other by little hooks that they have in their feet. By this means they can make ſuch figures in the air as they pleaſe. The king conceals himſelf thus; either out of care for his young, or perhaps through modeſty; for there is nothing that we may not imagine of the bees. But however he has not always been able to eſcape M. *Maraldi*'s eyes. He has perceived him to be followed by a court, moving with his ſolemn air, and depoſiting in 8 or 10 cells, one after another, ſo many little white worms, which are afterwards to be changed to bees. Whilſt this is doing, it appears by ſome particular motions of the bees which compoſe his retinue, that they careſs, applaud, or encourage him. After this he retires into the inner part of the hive, from whence he ſeldom comes out.

By the 8 or 10 worms, which he depoſits one after another, in the little time, and in the circumſtances that have been obſerved, we may judge of his fruitfulneſs during the reſt of the time, when he is not perceived; that is, during almoſt the whole year. It muſt needs be prodigious. When he is ſingle in a hive, which is moſt uſual, he is the only one that produces: there comes out of this hive in the compaſs of a year, one ſwarm at leaſt, which may conſiſt of 12 or 15,000 bees; ſometimes there come out 2 or 3; and yet it is as full at the end of the ſummer, as it was at the beginning of ſpring. A new ſwarm therefore, if it is the only one of the year, muſt conſiſt merely of the king's family, ſuppoſing there are none but young bees in it; and if there are any old ones in it, there remains in the hive an almoſt equal number of the young ones, which were produced by the king, which comes to the ſame thing. It is not probable that the king which comes out of the hive with the new ſwarm, has produced any part of the bees which accompany him. But if there comes in a year more than one ſwarm out of a hive, they muſt ſtill be new productions to be placed to the account of the old king, unleſs for fear of carrying his fruitfulneſs too far, we ſhould ſuſpect that he has produced more than one king; that there went out but one with the firſt ſwarm, and that the other, or two others remained in the hive, and produced their young. If this be ſo, a king may come forth with an entire new ſwarm produced by himſelf, and ſo be literally the father of his people; whereas the other kings will only be the brothers, becauſe they came from the ſame bee. Which way ſoever we take it, they have this particular privilege, that their king is given them by nature herſelf.

It remains to inquire from whence his fruitfulneſs is derived, and whether it is from any copulation. There is hardly any hive wherein we do not find ſome drones, and ſometimes ſeveral hundreds of them. They are made like the bees, only they are about $\frac{1}{3}$ longer and thicker, and have no ſting. They have none of the laborious temper of the bees, and remain abſolutely idle.

They go very little out of the hive, unleſs in very fine weather, and return again without bringing any thing with them. Not but that their veſicle is filled with honey, but they are ſuſpected to have robbed the hive of it, becauſe they are not ſeen to ſettle upon flowers. And even if they ſhould go to gather any, it would be only for themſelves, and not for the publick good; for M. *Maraldi* has found, that upon ſqueezing their veſicle, there came no honey out of it, as there does out of the bees; and ſo the drones are not capable of throwing it out again. We might imagine theſe animals to be the males of the great bee or king; and that they are ſuffered in the hive only becauſe their idleneſs would be ſufficiently recompenſed by this important function. And what would ſupport this opinion, is, that at the end of the ſummer the bees make war upon the drones without mercy, kill them, or drive them out of the hive without quarter, ſo that we do not know what becomes of them afterwards. One would think that the cauſe of their misfortune might be their being become abſolutely uſeleſs, becauſe they do not perform the work of generation in winter. But what cauſes a great deal of difficulty is, that M. *Maraldi* has ſeen ſome combs without drones in ſummer, and at a time when the cells were well ſupplied with little worms.

The myſtery of the generation of bees remains therefore pretty much concealed, but the care which they all take in common of their young ones which they have not brought forth, and which belong only to their king, is very viſible and remarkable. They ſeem to be regarded as children of the ſtate. They give each little worm in its cell ſome drops of liquor for its nouriſhment, and then make a waxen cover for the cell.

Theſe different operations have their regular times, and are without doubt for the uſes of the embyron. We leave the particulars of this to M. *Maraldi*, as alſo thoſe of the ſucceſſive growths of the worm, which being at laſt changed to a fly, pierces the lid of its cell, and after continuing ſome time in a languid ſtate, flies about with the reſt. It is remarkable, that the bees carry their frugality to ſuch a length, that they will not ſuffer this broken lid to be loſt. They take up the wax, and carry it into the common magazine, to be uſed anew; at the ſame time they give the cell its regular figure, if it has been altered, and put it in a condition to ſerve again for the ſame purpoſe, there have been worms ſometimes running in the ſame cell, in 3 months time.

The drones proceed from the king as well as the bees; there are in the combs cells larger than the reſt, intended for the worms which are to change to drones, and conſequently require more room; theſe worms are produced by the king with the ſame ceremony, and afterwards treated by the public with the ſame care, as thoſe which are to be bees; all is equal 'till the end of ſummer, but when the time is come for the bees to declare war againſt the drones, their fury extends even to thoſe which are yet but worms; they break the lids which they themſelves had put over them, and pull them out to kill them, and throw their little carcaſſes out of the hive, a change very difficult to be comprehended in ſo wiſe a nation.

We omit a great many particulars related by M. *Maraldi*, to mention all the wonders of this inſect would carry us too far; and how many inſects have their wonders yet unknown? and how many others have theirs, which will be unknown for ever?

An

An explanation of the figures in plate IV.

Fig. 1. The king, or rather queen of the bees in its natural bigneſs.

2. The drone in its natural ſtate.

3. The bee in its natural ſtate.

4. The trunk of the bee extended in length, and larger than nature, with the 4 branches a little ſeparated from one another to ſhew them the better.

5. The head of the bee to ſhew its jaws.

6. The leg of the bee magnified, detached from the body of the bee, and loaded with wax.

7. The baſe of the cell in a horizontal ſituation, to ſhew more plainly the figure of the egg immediately after it is laid, and in what manner it is uſually placed upon its baſe.

8. The baſe of the cell in its vertical and natural ſituation, with its egg changed into a worm or maggot, and ſurrounded with a ſmall quantity of liquor 4 days after it is hatched.

9. The worm inlarged 8 days after it is hatched.

10. The ſame worm 12 days after it is hatched, having changed its figure and ſituation.

11. The ſame worm changed into a *Chryſalis* larger than nature. It repreſents the bee while it is yet white and ſoft.

12. The figure of a cell detached from the reſt and ſeen on the outſide.

13. A part of a comb which ſhews in what manner the cells are ranged in the two oppoſite faces of the comb.

14. A piece of comb which repreſents the cells ſeen on the inſide, with the apertures ſtrengthened with a rim.

15. Several cells from which the ſides have been taken off only to ſhew their baſes. This figure ſhews how the baſes are ranged with regard to each other; and in what manner the two orders of cells are formed in the 2 faces of the comb. For the angle A repreſents the concave ſolid angle at the bottom of the cell in one face of the comb; the angle B, and the others of the ſame order ſhew the ſolid angle which is convex in the ſame face of the comb, but concave in the oppoſite face, and is found at the bottom of the cell oppoſite to the firſt.

16. Repreſents a canal, the origin of which is at A, where the 4 glandulous bodies are, and its extremity in B.

17. Repreſents a part of the ſame canal much larger than nature, to ſhew more diſtinctly the two wings which are at A, the bag B, and the 2 ligaments CC.

18. The bag A larger than in the preceding figure, to diſtinguiſh thoſe folds in which the ſeminal matter paſſes.

19. Is the ſame portion of the canal as in *fig.* 18. but ſeen on the other face, where are the 5 portions of black rings, and of the conſiſtence of horn, which embrace a part of the exterior circumference of the canal.

II. *On the progreſſive motion of ſome ſhell-fiſhes, or ſea-animals.*

This is the continuation of a ſubject begun in the hiſtory of 1710*. Natural hiſtory is immenſe, and that part of it which is expoſed to our eyes, is nothing almoſt in compariſon of what is more concealed, and cannot be diſcovered without a great deal of time, leiſure, patience, ſkill, and a ſort of eyes which every body is not endowed with.

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* See vol. III. page 321 of this abridgment.

The razor-fiſh is an animal incloſed in a ſhell, almoſt after the ſame manner as a knife or razor is in a round ſheath. This ſhell is formed of 2 halves of a hollow cylinder, cut along its *axis*, or length, and theſe two pieces are joined on both ſides by a membrane which ſuffers them to recede a little, and approach again. The animal which inhabits this cylindrical ſhell, keeps itſelf always plunged perpendicularly in the ſand, but always with its head uppermoſt. Its head is diſcovered, not by its ſhape, but by two tubes, which receive and reject the water neceſſary for its reſpiration. The purr which was mentioned in the place above quoted, and ſeveral other ſhell-fiſhes or ſea-animals have ſuch tubes. The lower part of the razor-fiſh is that which ſerves for its progreſſive and perpendicular motion, for it only plunges itſelf into the ſand, or raiſes itſelf a little above it. For this purpoſe it has a ſort of leg which it thruſts out of its ſhell when it pleaſes, and it is cylindrical quite to its lower extremity; or, when it is thruſt out, it becomes a ſort of ball, the diameter of which is greater than that of the cylinder. If the razor-fiſh has a mind to plunge, it thruſts out this whole leg, and conſequently engages in the ſand the great ball which terminates it, then it contracts the leg, the extremity of which being engaged in the ſand by a great ſurface, does not find it ſo eaſy to riſe again, as the ſhell does to go down. If the animal would raiſe itſelf, it only thruſts out at firſt the part which is to become a ball; and then it endeavours to prolong the reſt of the leg, or cylindrical part, and thruſt it out, and this part reſting upon the ball, cannot prolong it without making it go down, or puſhing the whole ſhell upwards. But it is more eaſy for the ſhell to riſe, than for the ball to go down; becauſe the ball reſts upon the ſand by too great a ſurface.

It is eaſy to cauſe theſe two motions in a razor-fiſh. When the ſea is retired, and has left the hole where it lodges uncovered, which is eaſily found by its ſhape, we need only throw a little ſalt upon it, and the razor-fiſh will immediately come half out. It is then eaſy to take it, but if we would ſee it plunge again into its hole, we need but touch it, and it is the ſame thing, if we miſs catching hold of it. After this, it is in vain to throw ſalt upon it, for it will not come out again. M. *de Reaumur*, the author of all theſe obſervations, thinks it is its averſion to the ſalt thrown upon it, and its endeavour to ſhake it off, and get rid of it, that makes it come out of its hole. For he has found, that upon putting ſalt on thoſe tubes or horns, with which it reſpires the water, the little cylinders placed end to end, of which it is formed, ſeparate at their joints which have been touched by the ſalt, and fall upon the ground, or need only to be touched very lightly to make them fall, which deſtroys an organ very neceſſary for the animal. It is ſurpriſing that ſalt ſhould be ſo injurious to an animal, which lives entirely in ſalt water.

The *dail* is another ſort of ſhell-fiſh, which is always found plunged in the ooze, or in the *banche*, which is a ſoft ſtone, but very hard in compariſon of the ooze; and M. *de Reaumur* proves it to be nothing but ooze hardened, and petrified by the viſcous part of the ſea-water. The figure of the *dail*, and of its hole, is almoſt that of a truncated cone, the ſmalleſt baſe of which is always uppermoſt, and conſequently the *dail* does not come out of this hole. It muſt have gone into it, or rather hollowed it when it was young, and afterwards have plunged it in farther, and augmented it in proportion as it grew. This is its whole progreſſive motion, which is only that of its growth; and therefore can be only of an extreme ſlowneſs. The inſtrument with which it bores is a pretty thick part, and made almoſt like a lozenge, and it thruſts it out at the lower end of its ſhell.

It muſt be imagined, that M. *de Reaumur* made uſe of ſome artifice to diſcover theſe ſorts of operations, which are performed only in obſcurity, and in ſecret. It was by holding a razor-fiſh in the air between his fingers, that he ſaw it thruſt its leg out of the ſhell, and make the ſame efforts that it would have made to plunge itſelf into the ſand; and it was by taking a *dail* out of its hole, and putting it into the ooze, that he ſaw the action of this part made like a lozenge.

All the young *dails* are in the ooze, and all the old ones in *banche*, which proves that the *banche* is ooze petrified. Very often the upper and greateſt part of the hole is *banche*, and the reſt is ooze. We may plainly ſee that the upper part, which receives the impreſſion of the ſea-waters more eaſily, muſt be the firſt that petrifies. It is highly probable that the *dails* live a long time, for the change of the ooze into *banche*, which is made during the life of a *dail*, muſt be made but ſlowly, and by inſenſible degrees.

The *dail* has alſo 2 tubes, with which it takes in the water, and throws it out again, and the length of theſe regulates the depth at which it keeps.

M. *de Reaumur* has obſerved a ſea-ſtar with 5 rays, like that mentioned in the *Hiſtory* of 1710*; but it has no legs to theſe rays, and differs alſo from the other in their being ſhaped like lizards tails, which is its characteriſtick. The 5 rays are themſelves the legs; the animal hooks two of them to the place to which it would advance, and trails itſelf upon theſe two, whilſt the oppoſite ray bending itſelf a contrary way, and reſting upon the ſand, puſhes the body of the ſtar-fiſh toward the ſame place. There are two others which remain uſeleſs, but they would not be ſo if the animal had a mind to turn either to the right or left; and thus we ſee how it might go on all ſides with equal facility, only by uſing 3 of its legs or rays, and leaving the 2 others at reſt. Perhaps alſo nature has given 5 legs to this animal, becauſe as M. *de Reaumur* has obſerved, they are very brittle, and have occaſion for a reſerve.

It is by a like mechaniſm that the ſea-urchin, or ſea-hedge-hog walks with 2,100 ſpines, with which its body is on all ſides ſurrounded. It draws itſelf with thoſe which are toward the ſame place to which it would go, and puſhes itſelf towards the ſame place with the oppoſite ones, all the others remain without action during that motion. Which way ſoever it would place its body, it has legs to go in that poſition. It has the mouth however commonly downwards, that it may feed. Beſides its 2,100 ſpines or legs, there are 1,300 horns, which are of the ſame uſe to it as the horns to a ſnail, or a ſtaff to a blind man, to feel the ground upon which it walks, and afterwards as anchors to a veſſel to fix and hook it where it pleaſes. It is plain, that as nature has ſet its whole body round with ſpines or prickles, the horns alſo ought to be prickly, ſince the one cannot be in action without the others. M. *de Reaumur* has ſubverted a fact related in the *Hiſtory* of 1709*, on the credit of an able man, which however was falſe. This difficulty of obſerving obſcure or complicated things ſufficiently juſtifies ſome miſtakes; but if miſtakes were inexcuſable, it would be ſtill more inexcuſable not to acknowledge them.

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* See Vol. III. page 323 of this abridgment.

An Explanation of the figures.

Plate IV. *fig.* 20. repreſents a heap of ſand, GGGGKKK, which muſt be imagined to be prolonged a good deal below KKK; in this heap are the figures 20, 21, 22.

Fig. 20. repreſents the aperture TTTT, *&c.* of the holes of the ſolen, called in *Engliſh* the ſheath, or razor-fiſh.

Fig. 21. ſhews a razor-fiſh riſing above the ſand, after the fiſher has thrown ſome ſalt into the aperture of one of the holes T, the fleſhy part OO is then purſed up.

Fig. 22. ſhews a razor-fiſh riſing without compulſion above the ſurface of the ſand, to reſpire the water; the part which then comes out of the ſhell, ſeems to be compoſed of 2 tubes ſet cloſe together, AHC, aHc, the firſt is bigger than the ſecond; Aa are the apertures of theſe tubes, which in OO, *fig.* 21, ſeem almoſt cloſed, becauſe the animal would willingly hinder the entrance of the ſalt. CC, HH, ZZ, repreſent the places where the different portions of which the part Aa, CC is compoſed, are united together.

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* Vol. III. Page 168 of this abridgment.

Fig. 23. is the part Aa, CC, of *fig.* 22, which we muſt ſuppoſed to have been detatched by the ſalt, applied at CC, Bb is the part which reſted upon CC.

Fig. 24. is a razor-fiſh laid upon the ſand; here we ſee how it prepares itſelf to begin its progreſſive motion. P is the extremity of the leg, which comes out of its ſhell, and is then flatted. In the ſame figure MMmm ſhew the membrane, which on one ſide is faſtened to the edges of the 2 pieces of the ſhell; towards LN there is another membrane, which joins together the two other edges of the ſame pieces, but this membrane could not be ſhewn here, it appears in *fig.* 25.

Fig. 25. ſhews alſo a razor-fiſh laid upon the ſand, but on another ſide than that of *fig.* 24. at L is the ſpring like that of the ſhells of oyſters, and muſcles, which faſtens the 2 pieces of the razor fiſh together; from this ſpring to the other extremity NN of the ſhell, there is a membrane LNN made in form of an iſoſceles triangle; at R is the leg of the razor-fiſh, already plunged in the ſand, and hooked.

Fig. 26. repreſents a razor-fiſh, which is ready to make one ſtep to plunge itſelf into the ſand, CCCC mark the ſhell; from the lower aperture of this ſhell proceeds the leg IRP, of which the part IR is cylindrical, at the end of this cylinder is the part P, which may be called the button of the leg, at OO are the ſame 2 tubes, but more contracted, which appear at Aa, *Fig.* 22.

Fig. 27. is a razor-fiſh of which the inner parts are ſhewn, for it has been opened, after having cut the membrane MMmm of *fig.* 24. in two, this very membrane is here wrinkled as may be ſeen in MmMm; and its ſpring alſo endeavours to fold it. L is the place where the leg LP is ſuſpended, which here has a very different form from that under which it appears in *fig*. 24, 25, & 26. It is compoſed of circular and longitudinal fibres, which ſerve to lengthen and contract it, to thicken and flatten it, according to the occaſions of the fiſh; at EEH appears the membrane or membranes which form the tubes AHC, aHc of *Fig*. 22. here they are folded, which is probably their natural ſtate.

Fig. 28. is that of one of the ſhell fiſhes called in *French Dails*, taken out of its hole. AGP is one of the two piees of it s ſhell, at ED is the elaſtic ligament, which faſtens the two great pieces together; DB is a third piece of the ſhell much ſmaller than the two others. At AICF is drawn a fleſhy tubular part, which the animal prolongs or contracts in different circumſtances; tho' it ſeems to be but one ſingle tube, it is in ſome ſort compoſed of two different tubes; a membrane, of which the extremity appears in C, like a ſort of partition, divides the tube from one end to the other into 2 equal parts; the bending AGP is called the baſe of the ſhell, the animal however never reſts upon this part of the ſhell; and it is called the baſe only becauſe it is oppoſite to the elaſtic ligament ED, and becauſe in another memoir, to expreſs ourſelves in a convenient manner, we have given the name of the baſe of the ſhell, to the part oppoſite to this ligament.

In *fig*. 29. is repreſented a bit of ſtone or *banche*, inhabited by the *dails*; this bit often conſiſts of ſtone from QQQ to III, and the reſt of ooze; OO, &c. are the apertures by which the *dails* ſend out the fleſhy tube, which they make uſe of to reſpire the water, at TTK there are ſome of theſe fleſhy tubes out of their hole. The *dail*, of which we ſee the end of the tube K, appears placed in its hole as it is there naturally, half the ſides of this hole is carried away, the part AALLHHPP is the part oppoſite to the part FEDB of *Fig.* 28; LLHH is a membrane, which joins together the two great pieces of the ſhell; S is a fleſhy part, with which it hollows the ooze, ZX is a hole of a younger *dail* than the *dail* KAAPP. V is the hole of another *dail*, of which we ſee but a part.

Fig. 30. ABCDC is a bit of ooze placed horizontally as it was in the bottom of the ſea. CDFE, FEGH, *&c.* are different horizontal *laminæ*, into which the bit of ooze divides in drying.

Fig. 31. repreſents the ſame bit of ooze as *fig.* 3. but placed in ſuch a manner, that the ſurfaces are vertical, which at the bottom of the ſea were horizontal; the different *laminæ* alſo, into which it divides, are in a vertical plane.

Fig. 32. repreſents one of thoſe ſea-ſtars, which we have called *ſtars with rays like lizards tails*, ſeen on the upper part. PTTRR are the rays of this ſtar. ABDCE is the maſs of its body, or if I may ſo ſpeak, of its back. The letters MM ſhew ſome of thoſe extremely narrow membranes, which the ſtar agitates in the water, and are hidden when it is out.

Fig. 33. is the ſame ſtar reverſed, its rays or legs, and its body are marked by the ſame letters with the preceding figure, that it may be ſeen underneath; at S is its mouth or ſucker.

Fig. 34. repreſents the skeleton of a ſea-urchin or ſea hedge-hog ſeen above; O is its ſuperior aperture. The TTTTT mark the five great triangles, filled with the eminences, with which its ſurface is adorned, The t t t t t mark the five ſmall triangles, and the 10 B's mark the perforated liſts, which ſeparate theſe triangles one from another. At mM one of theſe eminences, or *apophyſes* which are upon the skeleton is repreſented larger than nature.

Fig. 35. is the ſame skeleton of the ſea-urchin ſeen underneath; the aperture H is its mouth, at DD appear two ſorts of boney rings, out of each ring proceeds a tooth of the animal, and each of its teeth is alſo incloſed in a boney ſheath.

In *Fig.* 36. is a piece of the upper ſurface of the ſea-urchin, repreſented larger than nature, that the diſpoſition of the liſts of holes may appear better, and that the order of the holes, with which they are filled, may be perceived more diſtinctly. BB are two of theſe liſts, which are at the ſides of a ſmall triangle t; each liſt B is formed of two different rows of holes, ſome RR are compoſed of four holes, and the others S have but two. T is the great triangle, which follows the liſt.

Fig. 37. is alſo a skeleton, but a part of it has been taken off, to ſhew the inſide; we may obſerve in it the ſame diſtribution of the great and ſmall triangles, and of the perforated liſts; we have alſo marked theſe parts with the ſame letters as in the preceding figures; but there does not appear upon this inner ſurface any of the inequalities which are upon *fig.* 34, 35.

Fig. 38. repreſents a ſea-urchin in motion, the letters EE, &c. mark the ſpines with which it draws itſelf towards EE, &c. and KKK ſome of the ſpines with which it puſhes itſelf towards the ſame ſide, the letters eeee are much ſmaller ſpines; CCC, &c. are the horns with which it feels the bodies that preſent themſelves in its way. It may be obſerved that the ſurface of this ſea-urchin is in ſome meaſure divided into different triangles, like its skeleton.

Fig. 39. ſhews a ſea-urchin at reſt; it is reverſed; its mouth appears at BB, furniſhed with five teeth; at CCCCC there are ſeveral of its horns ſtuck againſt the ſtone PP; I is a horn ſeparated.

III. *On the declination of the needle, tranſlated by Mr.* Chambers

M. *Deliſle* has communicated ſeveral obſervations on the variation of the needle, which had been ſent him by intelligent perſons, from different parts of the kingdom, the reſult whereof is:

1ſt. That the variation is continually greater to the eaſtward of *Paris*, and ſmaller to the weſtward.

2dly. That from St. *Malo* to *Geneva*, which may be taken for the two extremities of *France*, in reſpect of longitude the difference of variation is only 1° ½.

3dly. That the variation which is now north-weſt and increaſes every year, has increaſed at *Geneva* much after the ſame rate as at *Paris*. From the year 1703 to the year 1711, that is about 15′ every year, and even that an irregularity obſerved at *Paris* in 1710, when the variation only increaſed 5′, was alſo found at *Geneva*.

4thly. That from 1706 to 1711, the variation has increaſed at ſeveral cities in *France*, much after the ſame manner as at *Paris*.

To ſee ſo many appearances of regularity ſo well maintained, 'tis hardly poſſible to reſiſt the expectation of a theory to come, tho' the example of ſeveral crude attempts, which have already been made, muſt induce us to have patience 'till a ſufficient number of obſervations have been made; M. *Deliſle* has given a little hiſtory of what has paſſed among the learned on this occaſion; we ſhall here only give a ſhort abridgment thereof, and the rather as a great part of what can be ſaid on this head, is already found in the notes of Father *Gouye*, printed in the memoirs for the year 1692. It may ſuffice for our preſent purpoſe, to relate what additions M. *Deliſle* has made thereto, whether they be hiſtorical or philoſophical.

The attractive faculty of the load-ſtone was known by the ancients, but that other power whereby it points towards the pole was not known 'till many ages after, the earlieſt author who ſpeaks of it being a *French* poet of the thirteen century, and its variation came 300 years after; the firſt who publiſh'd this was *Cabotto*, a *Venetian* navigator in 1549, tho' M. *Deliſle* has a manuſcript of a pilot at *Dieppe*, called *Crignon*, which he dedicated to the admiral *Chabot* in 1534, wherein mention is made of the variation of the needle; this novelty turned all the philoſophers againſt it, as it broke in upon their ſyſtems, in effect they obſtinately denied it; but it growing at length inconteſtable, they were forced to yield.

Hereupon it was obſerved however, that under the meridians of the *Azores* there was no variation; this made them conclude, that they had found a natural principle, for fixing the firſt meridian there, which hitherto had only been done arbitrarily, and conſequently not to the liking of every body; by the direction they found the load-ſtone at poles, and by its variation that theſe were different from thoſe of the earth, they therefore placed them where they pleaſed, with intire liberty which was the fruit of a want of obſervations.

They

Pl. IV. Vol. IV.

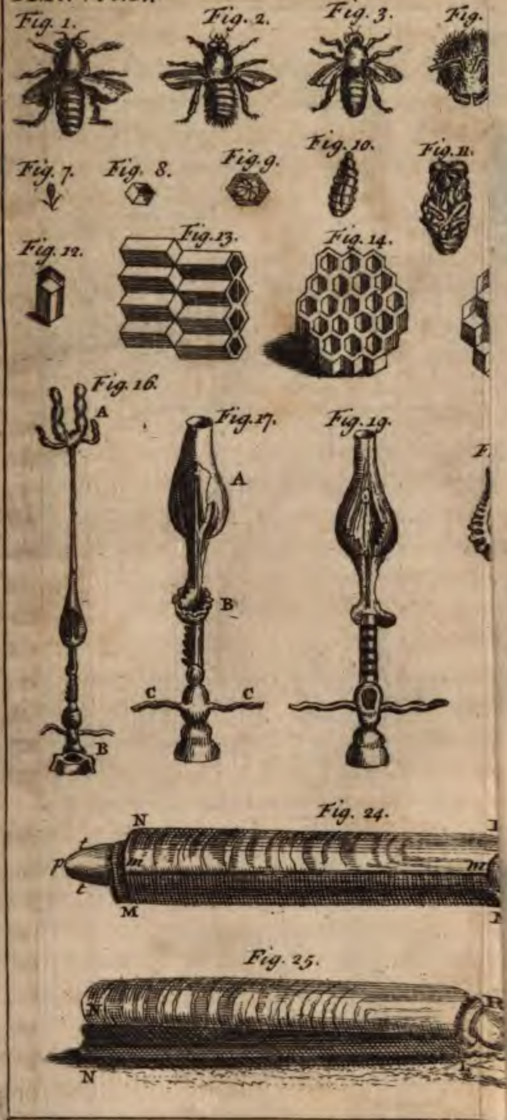
Fig. 1.
Fig. 2.
Fig. 3.
Fig.
Fig. 7.
Fig. 8.
Fig. 9.
Fig. 10.
Fig. 11.
Fig. 12.
Fig. 13.
Fig. 14.
Fig. 16.
A
B
Fig. 17.
A
B
C
C
Fig. 19.
F
N
Fig. 24.
t
p
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Fig. 25.
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N

But they came at length to the knowledge of two new meridians, void of variation, one paſſing through a cape ſituate near the cape of *Good-Hope*, and for that reaſon called the cape of *Needles*; and the other thro' *Canton* in *China*; accordingly they determined the angles of interſection of theſe ſuppoſedly fixed meridians, the preſumption being ſtill on the ſide of immobility, and filled up their intervals with other meridians, under which there was a variation, and even ranging them proportionably on the ſuppoſition that every thing proceeded according to the moſt natural order, but all this was direct precipitation.

Soon after it was diſcovered, and principally by M. *Gaſſendi*, that the variation of the needle had another variation; that it was changing from time to time in the ſame place, and this continually. A diſcovery which overturned all they had built. By this inſtance as well as by infinite others, it appears how ſlow our progreſs is, that there is always a conſiderable diſtance between one diſcovery and another, and yet that theſe diſtances grow continually ſmaller and ſmaller the nearer we deſcend to the preſent time.

Of the ruins of ſo many ſyſtems, nothing now remains but that propoſed by Dr. *Halley*, who has drawn a line round the globe of the earth, which is exempt from variation; this line is neither a meridian, nor a circle, but a very irregular curve. The variation of the declination in each place require this line to be movable; and we already find by ſufficient experience, that it is ſo. 'Tis probable likewiſe that it changes its figure, by reaſon the variations of the declination in one place are not always proportionable to thoſe of another; this line, as drawn by Dr. *Halley*, paſſes on th one ſide thro' the north-ſea, and the *Bermudas* iſlands; and on the other, through *China* about 100 leagues to the eaſt of *Canton.*

M. *Deliſle* from ſome obſervations made by a *French* ſhip which went through the *South-ſea* to *China*, and was the firſt ſhip of that nation which ſteered that courſe, has found another line of no variation which traverſes the *South-ſea* from north to ſouth, much like a meridian; this makes a conſiderable addition to the ſyſtem, as well as the chart of Dr. *Halley*, wherein the *South-ſea* was intirely wanting.

There is one remarkable difference between the 2 lines or portions of lines drawn by Dr. *Halley* and M. *Deliſle*, when compared together, for to the eaſtward of the line of no variation which paſſes thro' the *Bermudas* iſlands, the declination is north-weſt, and weſtward of the ſame line north-eaſt, the contrary whereof obtains in the line which paſſes thro' *China*; whereas in the other line, traverſing the *South-ſea*, the declination is north-eaſt on both ſides; this difference gives each of them its ſeveral character, which, if it be invariable, will always help to diſtinguiſh them whitherſoever they go.

M. *Deliſle* has taken a deal of pains to diſcover ſome traces of the motion which theſe three lines muſt have had to arrive at their preſent poſition; and is convinced that the line which now paſſes thro' the *Bermudas* iſlands is the ſame with that which about the year 1600 paſſed through the cape *des Aiguilles*, on which ſuppoſition it muſt have moved from eaſt to weſt, though not parallelly to itſelf; for in the year 1600 it was almoſt a meridian which paſſed through the cape *des Aiguilles*, the *Morea*, and the *North-Cape*;

but from that time to this, it has moved 1400 leagues by its northern end, and only 500 by its ſouthern one; ſo that its preſent ſituation is north-weſt, and ſouth-eaſt; being much inclined to its antient poſition, its northern end paſſed thro' *Vienna* in 1638, thro' *Paris* in 1666, and thro' *London* in 1667, which places were all accordingly free from variation in thoſe years. M. *Deliſle* is likewiſe of opinion, that the line which is now 100 leagues eaſtward of *Canton*, is the ſame which in 1600 paſſed through that city; whence it follows, that it has travelled from weſt to eaſt, contrary to the other, and at a much ſlower rate. If theſe 2 lines continue their courſe, they will at length meet one another, the conſequence whereof it is not eaſy to foreſee.

As we have no antient obſervations of the *South-ſea*, it would be preſumptuous to advance any thing as certain, relating to the line which paſſes through it, only it might perhaps be ſuſpected to be the ſame which formerly paſſed through the *Azores*, and which has moved from eaſt to weſt.

The difference of the declination in different places M. *Deliſle* obſerves, is by no means proportionable to the diſtances of thoſe places from their line of no variation, that is, to a degree of difference in the declination of the needle, correſpond very different diſtances on the ſurface of the earth. In Dr. *Halley*'s chart, the greateſt of thoſe diſtances is 130 leagues, and the ſmalleſt 15; but then he has only extended his chart to the 60th degree of northern latitude, and M. *Deliſle* who has obſervations made 20°——more northerly, finding that there are ſome degrees of difference in declination which only afford 8 leagues diſtance; and hitherto it appears, that as the climate is the more northern, the ſmaller diſtance correſponds to a degree.

Nor does the declination vary equally in equal times at the ſame place. At *Quebeck* M. *Caſſini* found, that it had only vary'd ½ a degree in 37 years, whereas by other obſervations in M. *Deliſle*'s hands it has varied a whole degree in 11 years.

But notwithſtanding all this, we perceive a ſort of progreſſion, and ſome appearance of regularity which is enough to prevent a philoſopher from being diſcouraged at the ſight of ſo many ſeeming anomolies.

IV. *Of a cavern which is exceedingly cold in ſummer.*

It would be no great ſurprize to moſt people, if you ſhould tell them, that in a ſubterraneous place, a cave for example, it is warm in winter, and cold in ſummer, which they have met with a hundred times. It is a paradox however to philoſophers, who know this experiment to be fallacious, and that in reality it is hotter in a cave in ſummer, than in winter; but that the difference between the heat and cold is not near ſo great as in the exterior air; and that this inequality of difference makes the cave appear warm in winter, when one goes into it from a colder air, or cold in ſummer, when one goes into it from a warmer air. Therefore none but philoſophers can be ſurpriſed at a cavern of *Franche Comté*, where it is really very cold in ſummer.

This cavern is 5 leagues to the eaſt of *Beſançon*, in that part of the province commonly called *Montagne*, and in a wood near the village of *Chaux*. It it at the foot of a rock 15 feet high;

it is 80 feet deep, 140 in length from the entrance to the oppoſite ſide, and 122 in breadth. It was in *Sept.* 1711, that M. *Billerez*, profeſſor of anatomy and botany in the univerſity of *Beſançon*, who ſent this account to the academy, went down to examine it. He found, that the bottom of the den, which is flat, was ſtill covered with ice, which was beginning to melt; and he ſays, three pyramids of ice 15 or 20 feet high, and 5 or 6 broad, which were now alſo much diminiſhed. There began to come out of the top of the entrance a fog, which proceeds from it all the winter, and accompanies the thawing of this repoſitory of ice. But the cold was ſtill ſo great, that unleſs they walked and agitated themſelves, there was no ſtaying ½ an hour without ſhivering, and a thermometer, which out of the cavern was at 60 degrees, fell at 10; that is, to 10 degrees below a very great cold. The ice of this grotto is harder than that of the rivers, is mixed with fewer bubbles of air, and melts with more difficulty. It is the more in quantity, as the ſummer is hotter.

M. *Billerez* has found the cauſe of this *phænomenon*, by obſerving that the lands in the neighbourhood, and eſpecially thoſe above the vault, are full of a nitrous ſalt, or natural ſal armoniac. Theſe ſalts being put in motion by the heat of the ſummer, mix more eaſily with the waters, which flowing through the earth, and through the clefts of the rock, penetrate quite into the grotto. This mixture freezes them exactly in the ſame manner as the artificial ice is made, and the cavern is in great, what the veſſel in this operation is in little. Some coagulations, or ſtony incruſtations, which are found, eſpecially oppoſite to the opening expoſed to the north, by which more nitrous particles of air might enter, confirm this ſyſtem ſtill more. It is ſaid, that there are rivers in *China* which freeze in ſummer for the ſame reaſon.

V. *Of a ſpring which makes the teeth drop out.*

At *Senliſſes*, a village near *Chevreuſe*, ſituated in a valley at the bottom of a hill, there is a publick ſpring, which makes the teeth drop out, without any defluxion, pain, or bleeding. This effect cannot be aſcribed to any thing elſe, for the air is very good and temperate, and the inhabitants are more robuſt and healthy than elſewhere; only above half of them have loſt their teeth. At firſt they ſhake in the mouth for ſeveral months like the clapper of a bell, and afterwards they fall out very naturally. The water accuſed of this diſorder is very briſk, it is very cold when drank at the well head,it is found to be hard when uſed in the kitchen, and it is ſaid to gripe thoſe who are not uſed to it. M. *Aubry*, the curate of the place, who ſent a barrel of this water to M. *Couplet*, with an ample account of all that relates to it, ſays, he had been adviſed not to make uſe of it till after boiling, which would cauſe the ill quality of it to evaporate. He takes it to be a mineral, and conjectures, that it even contains ſome mercury.

M. *Lemery* having examined it every way, and ſubjected it to all the chymical trials, has not been able to diſcover any thing particular in it. Only in evaporating a gallon of it over a gentle fire, there remained 12 grains of an acrid, fixed, alkaline ſalt, which ſeems to be very little for ſuch a quantity of water. He found no indication of mercury. Beſides, he has given water, in which quickſilver had been infuſed or boiled, to children for the worms; but he never found that it looſened the the teeth. The ill quality therefore of the water of *Senliſſes* is too fine and ſubtile, to make a ſenſible diſcovery of itſelf to us.

It has been more eaſy for M. *Lemery* to find a like example. He has recollected, that *Vitruvius* ſpeaks of a ſpring at *Suſa* in *Perſia*, the water of which makes the teeth of thoſe who drink it drop out; and it is remarkable, that he has ſeen at *Paris* a *Perſian* born in that very city of *Suſa*, who could take 7 or 8 teeth out of his mouth with his hand, and put them in again with the ſame eaſe. He had the ſcurvy indeed; and perhaps the ſpring of *Senliſſes* would give that diſeaſe, if the goodneſs of the air, and other favourable circumſtances did not hinder.

VI. *Of the riſing of the ſea in the ſtreights of* Dover, *when the tide ebbs.*

F. *Gouye* ſays, that a ſailor had obſerved with the plummet in the *Pas de Calais*, or ſtreights of *Dover*, that the ſea roſe at the time of the reflux. The reaſon of a *phænomenon*, which appears ſo odd, is that the waters, which retire from the coaſts of *England*, meeting with thoſe which retire at the ſame time from the coaſts of *France*, ſwell againſt each other, and raiſe the middle of the ſtreight.

VII. *On the principal organ of viſion, and on the ſtructure of the optic nerve.*

The experiment of the cat immerged in water, had led M. *Mery* to a new explanation of the motions of the *iris* of the eye; the ſame experiment had taught him that the *retina* is as tranſparent as the humours of the eye themſelves; and thence he had concluded, that it was not made to receive the picture of the objects, and that the *choroides* which is opake and placed behind it, is fitter for this office: But he had only hinted this thought, and did not pretend to go to the bottom of a queſtion, which had before been handled with great ſubtilty in 1668, between Meſſ. *Mariotte*, *Pecquet*, and *Perraut*. M. *Mariotte*, having made a curious diſcovery of a place in the bottom of the eye, where viſion failed, maintained that the *choroides* is the principal organ of it. His obſervation is known to all, who have the leaſt tincture of natural philoſophy.

M. *de la Hire* having given another explanation of the experiment of the cat, than what M. *Mery* had given, occaſionally took the part of the *retina* againſt M. *Mery*, who ſubſtituted the *choroides* in its room.

M. *Mery* anſwered in the firſt place to what related to the motions of the *iris*, the firſt ſubject of the whole diſpute, and afterwards he came to the queſtion of the *retina* and *choroides*.

Tho' M. *Mariotte*, who was the firſt that maintained the rights of the *choroides*, has very well ſupported them, and ſeems even to have remained in poſſeſſion of the field of battle, having anſwered every thing, and written laſt, it muſt be confeſſed that the *retina* has remained in poſſeſſion of being the principal organ of ſight ; as this does not effect the general ſyſtem of viſion, perhaps thoſe who doubt upon this queſtion, or thoſe who are perſuaded in favour of the *choroides*, do not conſider how difficult it is to change the old and common ſtyle, by which the *retina* is eſtabliſhed. However, as exactneſs cannot be abſolutely indifferent with regard to truth, we ſhall here relate what is moſt important and new in this diſpute, for we ſhall avoid repeating what was ſaid in M. *Mariotte's* time, of which the publick may eaſily be informed.

The principal organ of ſight is that whereon the image of the objects is painted, that is, which receives the *vertices* of different cones of rays, ſent from different luminous or enlightened points, this organ muſt alſo be ſenſible.

The *retina* is a membrane formed by the expanſion of the optic nerve, which is opened into ſmall ſlender filaments, white like the nerve ; behind the *retina* is the *choroides*, which involves it, being another membrane, which is a continuation and extenſion of the *pia mater* ; it is black in men, birds, and ſome other animals, but in ſeveral *ſpecies* it has colours, and thoſe very lively : it will not be amiſs to obſerve that the *retina* and the *choroides* are alſo involved by the opake *cornea*, which is a continuation of the *dura mater*.

It has appeared to the philoſophers, that the *retina* had all the characters of the principal organ of ſight. It is placed in the *focus* of the refractions of the humours of the eye, and conſequently receives the *vertices* of the cones of the rays ; it is very fine, and conſequently very ſenſible, or rather ſenſible to very fine impreſſions, ſuch as thoſe of the rays; it draws its origin from a nerve, and is it-

itſelf quite nervous, and they are perſuaded that the nerves are the vehicle of all the ſenſations; laſtly, it communicates with the ſubſtance of the brain, where all ſenſations ſeem to terminate; as for the *choroides*, they have either been in no great doubt about its uſe, or thought that it ſtopped the rays, which the great fineneſs of the *retina* would ſuffer to paſs, and that it did with regard to the *retina*, what tin does with regard to a looking-glaſs, eſpecially in thoſe animals where it is black, becauſe black abſorbs the light, and all that could paſs thro' the *retina* would only trouble the viſion, if it was not deadned.

The experiment of the cat immerged in water, has cauſed M. *Mery* to have different notions: he ſaw that the *retina* abſolutely diſappeared, as well as all the humours of the eye, but that the *choroides* appeared very diſtinctly, and even with the lively colours, which it has in that animal; thence he judged the *retina* to be as tranſparent as the humours, and the *choroides* on the contrary to be opake, and conſequently that the *retina* was not fit to terminate the cones of the rays, or to receive the images of the objects; that the light ought to go through it, and could not ſtop but on the *choroides* which then became the principal organ of viſion. The black colour of the *choroides* in man is alſo very favourable to this opinion; it not only agrees with the principal organ of viſion, that the action of light ſhould be terminated when it arrives there, and it is certain that this action is terminated in black, which abſorbs the rays, and does not reflect them, but it alſo agrees with this organ, that the action of light ſhould be ſtronger there than any where elſe; and it is certain alſo, that as light is engaged in a black body, and does not come out of it, it cauſes a greater vibration therein. Thence it comes that black bodies are more eaſily ſet on fire by the burning-glaſs than white ones, every thing elſe being equal. The tranſparence of the *retina*, and the opacity of the *choroides* were not ſo certainly known in the time of M. *Mariotte*.

He judged rightly, that the poſition of the *choroides* behind the *retina* made for his opinion, but he did not draw ſo great an advantage from it as M. *Mery* did, who has obſerved in the other ſenſes the ſame poſition of the principal organ behind a mean organ, which gives a happy and concluſive analogy. The cuticle, which is extended over the whole ſkin is the mean organ of feeling, of which the ſkin is the principal organ. It is the ſame thing with regard to the ſmell and taſte, which, with regard to the diſpoſition of the organs, are nothing but feeling. The *membrana tympani* is a membrane extended before the *tympanum* or drum of the ear, and cloſes it; and by its means the agitations of the air are tranſmitted to the *lamina ſpiralis*, the principal organ of hearing, incloſed in the labyrinth, beyond the *tympanum*. It is true, the *membrana tympani* is not immediately applied to the *lamina ſpiralis*, but it is placed before it, which is ſufficient for the analogy. The *retina* therefore will be alſo no more than a mean organ, which will probably ſerve to hinder too great an impreſſion of the light on the *choroides*, or to preſerve it, which is the ſole uſe aſcribed to the cuticle, with regard to the ſkin.

But, according to M. *Mery*, there is more than all that. The *retina* is inſenſible, becauſe it derives its origin from the medullary ſubſtance of the brain, which is inſenſible alſo; and the *choroides*,

roides, on the contrary, is very ſenſible, becauſe it ariſes from the *pia mater*, which is certainly very ſenſible.

This proof, which ſome queſtioned, engaged M. *Mery* in a more particular examination of the ſtructure of the organs of ſight. He not only ſhewed the academy, by diſſection, that the *retina* and the *choroides* had the origins which he had pointed out, but added alſo an important diſcovery concerning the optic nerve. This nerve is not compoſed of filaments, as has been thought, and as the other nerves are; it is only a marrow incloſed in a canal, which may be eaſily made to come out. So long as the nerve is within the ſkull, the canal is formed only of the *pia mater*, and the marrow is contained in it in a lump: but the ſame nerve, when it enters the orbit of the eye, takes a ſecond covering, which comes to it from the *dura mater*, and is the *cornea*; it is a new canal, which covers the firſt externally, and there the marrow is incloſed in an infinite number of little cells, which communicate with each other. It is alſo with more difficulty ſqueezed out of this ſecond part of the optic nerve, than out of the firſt. The cells of the marrow have alſo a relation to the cavernous bodies.

The ſtructure of the optic nerve, unknown till now, does not permit the *retina* any longer to be a membrane; it is no more than a dilatation of the marrow, wrapped up with two membranes, and marrow is not a proper ſubſtance to be the ſeat of a ſenſation. We hardly conceive that it can ſerve for any other purpoſe, than to filtrate or carry the neceſſary ſpirits; but the vibration of the ſenſe itſelf muſt be made upon a part more ſuſceptible of a ſtrong impreſſion, more ſolid, and more firm; and it has always been thought, that none but nervous filaments could receive this vibration, eſpecially as they have always been allowed alſo to communicate it to the brain, the centre of the whole.

If the new ſtructure of the optic nerve obliges us to remove to the *choroides* the office aſcribed to the *retina*, it will greatly diſorder all the common ideas, either viſion muſt abſolutely terminate in the *choroides*, without going any farther; or if it does go farther, it ſpreads itſelf through the whole *pia mater*. It muſt be confeſſed, that theſe conſequences have their inconveniences; but it is true alſo, that whatſoever ſyſtem we take, if we purſue the ſenſations to the end, and till they reach the ſoul, we loſe ourſelves, and fall into the immenſe *chaos*, which is between the body and the ſoul.

VIII. *On the experiment of the eyes of the cat immerged in water.*

We ſaw in the preceding article, the principal part of the diſpute between M. *de la Hire* and M. *Mery*, on the cat immerged in water. It has been another queſtion between them, why the bottom of the cat's eyes appeared ſtrongly illuminated in the water, and abſolutely diſappeared in the air. M. *Mery* pretended, that when the animal was under the water, there entered more light into its eyes, becauſe its *cornea* was more flatted, and in ſome meaſure wrinkled by the moiſture. But M. *de la Hire* has given a pretty natural reaſon for it, taken from the principles of opticks.

The *cornea* being in the air does the office of a mirrour, becauſe it is ſmooth; and of a convex mirrour, becauſe of its figure. It has its *focus* therefore beyond itſelf, which is alſo very vivid, and it reflects their own image to thoſe who view it, which by its vivacity hinders them from ſeeing any other object beyond the *cornea*. But when the ſame organ is in the water, the little difference, that there is between its denſity, and that of the water, makes them be phyſically homogeneous; it is therefore no longer a convex mirrour, and it is only inſtead of a plain ſurface of water, and we ſee thro' it all that we ſhould have ſeen thro' the water.

IX. *On the reproduction of ſome parts of crayfiſhes, lobſters,* &c.

Tho' the ſyſtem of the animal quite formed in the egg renders the generation of it intelligible, yet it does not ceaſe to be very marvellous. But that in the room of an organical part which has been cut off, there ſhould grow another quite like it is a ſecond wonder, of a different order from the firſt, and out of the ſyſtem of the eggs. And indeed the philoſophers have refuſed to believe ſo ſurpriſing a fact upon the credit of the vulgar, and they muſt be allowed to be excuſable, if the ſtudy of nature ought to give them any confidence in their knowledge.

It is of the cray-fiſhes, crabs, and lobſters, that the fiſhermen have ſaid, that when they have loſt any leg or claw, there came another in the room of it; and M. *de Reaumur* has had the curioſity, the ſkill, and the patience to verify it; and this is the reſult of the obſervations which he has made chiefly on the cray-fiſhes.

Theſe animals have 2 great legs before, terminated by 2 pinchers. Each of theſe legs has 5 articulations, or joints; and I ſuppoſe with M. *de Reaumur*, that they are reckoned from the extremity of the leg, where the pinchers are. When the legs of the cray-fiſhes are broken by any accident, in walking, which is pretty common, they are always broken at a ſuture, which is near the 4th articulation, and in time there is renewed exactly what they have loſt; that is, a part of the leg which has 4 articulations; the firſt of which has the 2 pinchers, ſo that the loſs is entirely repaired.

If the great leg of a lobſter is broken deſignedly at the 4th or 5th articulation, the part taken off is always reſtored, but not if the amputation was made at the 1ſt, 2d, or 3d, articulation. The reproduction is then very rare, if things remained in this ſtate; but what is very ſurpriſing, they do not remain ſo. If at the end of ſome days we examine the cray-fiſhes, which have the legs broken at theſe unfortunate and barren articulations, we find they have them all broken at the 4th articulation; and we may ſuſpect them to have performed this operation themſelves, being ſure perhaps of having a new leg. The reproduction is beſt performed at this 4th articulation.

The reproduced part is not only quite like, but equal at the end of a certain time to that which was amputated. Hence it comes, that we ſee many cray-fiſhes which have their 2 great legs unequal in various proportions. This ſhews the age of the ſmalleſt.

If a reproduced part is amputated, there is a new reproduction formed.

The ſummer, which is the only time of the year, when the cray-fiſhes eat, is the moſt favourable ſeaſon for the reproduction. It is then performed in 4 or 5 weeks; whereas it would hardly be performed in 8 or 9 months in another ſeaſon.

The ſmall legs grow again like the great ones, but more rarely and more ſlowly.

The horns grow again alſo.

If we add to this all that has been ſaid of the cray-fiſhes in 1709*, we ſhall find that they furniſh ſeveral rare *phænomena.* It appears in general, that the moſt wonderful of all animals, with regard to mechaniſm, are thoſe which reſemble us the leaſt.

An explanation of the figures.

Tho' the cray-fiſh is an animal pretty well known, yet we have thought fit to engrave it, in order to ſhew the poſition of its different parts.

Plate V. *Fig.* 1. repreſents a cray-fiſh lying on its back, that the poſition of its legs may be ſeen.

P p 1, 2, 3, 4, 5, are its great claws, P p are the two pinchers: the little pincher p is articulated in p. At 1, is the firſt articulation of the leg; at 2, the ſecond; at 3, the third; at 4, the fourth; at 5, the fifth, or the place where it enters into the body of the animal.

Fig. 2. is a part of a cray-fiſh repreſented larger than that of the preceding figure, for the better diſtinguiſhing of the articulations and ſutures of the legs. The ſame letters of the preceding figure mark the ſame parts. P p the two pinchers 1, 2, 3, 4, 5, the five different articulations S is the ſuture where the leg of the crayfiſh naturally breaks, and where when broken, it is ſooneſt reproduced. This ſuture is more diſtinguiſhed than the reſt in the figure, becauſe it was neceſſary to ſhew it; but it is no more ſenſible in the cray-fiſh than in other ſutures; b c d e is that part which we have called the great leg of the cray-fiſh: b c d e are the different joints of it.

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* Vol. III. p. 244. of this abrigment.

Fig. 3, 4, 5, 6, 7, mark the different terms of growth of a leg broken at the ſuture S of *fig.* 2. Each of theſe figures is what remained to the leg from the fifth joint. 5 marks in all theſe figures the place where this fifth joint was.

Fig. 3. A ſhews the end of the leg juſt as it appears immediately after being broken, or a day or two afterwards.

Fig. 4. B marks the end of the leg, when the membrane which covers it again, has taken a figure a little convex.

Fig. 5. C ſhews the little fleſhy cones which come out of the end of this leg.

Fig. 2. D is the ſame cone conſidered ſome days later; it is more long.

Fig. 6. E is the little fleſhy cone, which begins to bend.

Fig 7. FGH is the ſame part of the leg ready to grow. It is then bent at G; its poſition is like that of one of the legs of the cray-fiſh laid upon the back *fig.* 1. tho' the leg in this figure is ſtill wrapped up in a membrane, yet the articulations are diſtinguiſhed thro' it. They may be ſeen thro' the tranſparence of the membrane. If we look at the end H againſt a ſtrong light, we ſhall perceive the ſeparation of the 2 pinchers as it is repreſented in *fig.* 8.

Fig. 10. repreſents a cray-fiſh in the ſituation in which it often is when it walks; TTTT is the great table of ſhell which raiſes itſelf firſt, when the cray-fiſh begins to mew; t t t, *&c.* are the little tables which hold together by different membranes; they are joined together in the ſame manner when the craw-fiſh has mewed.

Fig. 11. is the part of the ſtomach of the cray-fiſh, where the three teeth are, and the cartilages which ſuſtain them. The teeth of the middle B is of a different figure from that of the teeth DD.

AN
ABRIDGMENT
OF THE
PHILOSOPHICAL MEMOIRS of the ROYAL ACADEMY of SCIENCES at *Paris*, for the Year 1712.

I. *Obſervations on the rain, and upon the theemometer and barometer at the royal obſervatory during the year* 1711, *by M.* de la Hire*.

THE quantity of rain-water, and melted ſnow, was in

	Lin.		*Lin.*
Jan.	8 $\frac{1}{4}$	July	51 $\frac{1}{8}$
Feb.	51 $\frac{2}{8}$	Aug.	20 $\frac{1}{8}$
March	18	Sept.	24 $\frac{1}{2}$
April	20 $\frac{2}{8}$	Oct.	31 $\frac{1}{4}$
May	32 $\frac{1}{8}$	Novem.	21
June	8 $\frac{3}{8}$	Dec.	15 $\frac{3}{4}$

The ſum of the height of the water of the whole year was 302 lines, or 25 inches, 2 lines, which is much more than the mean years, which afford us only about 19 inches. It is pretty extraordinary, that this year ſhould afford us ſo much water, tho' it rained very little in *June* and *Auguſt*, which with *July*, commonly furniſh as much as the 9 other months together; and the more as it did not rain at all from *Sept.* 3, to 19; and from *Sept.* 19, when it rained 11 lines to *Oct.* 19, it rained but 2 or 3 lines. But the great ſnows of *Feb.* with the rains that followed them toward the end of the month, gave all at once a great quantity of water, which cauſed a conſiderable overflowing of the river; but it was not ſo great as that which happened in 1658 in *Feb.* the height of which is marked in the cloiſter of the *celeſtins* at *Paris.*

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* Jan. 9. 1702.

We cannot determine any thing certain on the height of the water to be furniſhed by a certain quantity of ſnow, for ſome is very rare, and ſome very much condenſed.

July 28, 29, and 30, furniſhed about 31 lines of water; and this was the greateſt continual rain of the whole year. There was a little ſtorm on the 28th in the evening.

My thermometer was at the higheſt at 62 parts ⅔, *June* 16, at ſun-riſing; and at two in the afternoon it was at 73 ½; which does not mark a very great heat; for I have ſeen it riſe to 80. *July* 10, when the heats are uſually greateſt, it was but in its mean ſtate about ſun-riſing, which is the time when I make all theſe obſervations. This very thermometer was at the loweſt at 20 parts, *Feb.* 15; but two days afterwards it roſe again to 36. The mean ſtate of the air marked by this thermometer, as it is at the bottom of the caves of the obſervatory, when it always continues at the ſame height, is 48 of its parts or degrees. It begins only to freeze in the country, when this thermometer is at 32; ſo that from the mean ſtate to froſt, it falls only 16 parts; and conſequently the degree of heat of the air, which ſhall be as much above the mean ſtate, as the degree of the beginning of the froſt is below it, will be 64, as it was this year nearly about the morning of the day of the greateſt heat; and the greateſt heat of this day at 2 in the afternoon, when the thermometer marked 73 ½, was almoſt as much above the mean ſtate, as the greateſt cold of the year was below it, when the thermometer was at 20, at the ſun-riſing, the coldeſt time of the whole day.

The thermometer which I make uſe of every day for my obſervations, is a ſimple barometer, which is placed at the top of the great hall of the obſervatory, wherein the quickſilver always keeps 3 lines lower than that which M. *Picard* made uſe of, and in which he perceived light upon agitating the quickſilver in the tube, which was a new *phænomenon*. I cannot ſuſpect that there is any air in mine; for it gives light like the other, and I have filled it with a great deal of care. It muſt be obſerved, that in order to have the true height of the quickſilver in the tube, the frame of the barometer ſhould be a little ſhaken againſt the wall where it is hung.

My barometer was at the higheſt at 28 inches, 5 lines, *Jan.* 21; the ſky was clear, with a moderate north wind near the ground; but the upper wind was eaſt. And during this whole month, there was but little rain, and the barometer was always very high; for half this month it was above 28 inches. The ſame barometer was at the loweſt at 26 inches, 9 lines, $\frac{1}{3}$, *Dec.* 10; with a very violent hurricane, the wind being toward the ſouth with but little rain. Thus the difference between the higheſt and loweſt ſtate of the barometer was 1 inch, 7 lines, $\frac{2}{3}$, which is a little more than the common ſtate; that is, 1 inch, 4 lines.

I obſerve in general, that during this whole year, when the barometer was at 28 inches, or thereabout, which happened pretty often, it has rained little or nothing, which agrees with the general opinion, that the barometer predicts the ſe-

ſerenity of the air; and when it was at the loweſt, there was always a good deal of rain and ſnow, as it happened in *Feb.* This rule however is not ſo certain, as to have no exceptions, for about the end of *July*, when it rained conſiderably, as I have already obſerved, the barometer was about 27 inches, 8 lines; which may happen from particular cauſes, which are not common, as a ſudden ſtorm, when we often ſee 2 contrary winds, which having different directions either upwards or downwards, and laſting but a little while, make irregular impreſſions on the quickſilver in the barometer.

The winds were this year as uſual in this country, very often toward the ſouth-weſt.

Oct. 6, at eight in the evening an earthquake was perceived only in my apartment at the obſervatory; and one of the principal ſigns of it was, that the great rings of a copper ciſtern ſtruck againſt the ciſtern, made a good deal of noiſe, and continued a long time in motion, which was obſerved by all who were in the place: but I did not mention it then, becauſe I ſuſpected that this ciſtern, which had a good deal of water in it, might have ſlipped a little upon its frame, and that a ſmall motion of the water might have given one to the ciſtern, ſufficient to make the rings ſtrike againſt the body. But ſome days afterwards we received letters out of the country, 30 leagues from *Paris*, by which we have been informed of an earthquake which was felt there, and had terrified the people of the place; and it was on the ſame day, and at the ſame hour, that we perceived it at the obſervatory. We have had accounts of it alſo from other places, where it was very conſiderable.

Dec. 30, 1711, I found the declination of the needle to be 10° 50′ toward the weſt as it was the preceding year; but it muſt be obſerved, thatat the end of the year 1709, it was but 10° 15′; and conſequently from 1709 to 1710, the variation has been 35 minutes, which was about double of what it was obſerved to be for ſome years; but 1711 has ſet it right, for we have but 35 minutes difference for 2 years. We cannot however ſuſpect any error in theſe obſervations, for we always make them with very great care, uſing the ſame needle, which is 8 inches long, and applying one ſide of the ſquare box, in which it is incloſed, againſt one of the faces of a great ſtone pillar at the bottom of the terraſs of the obſervatory. We are aſſured of the poſition of the face of this pillar, by ſeveral very exact obſervations, that it looks exactly to the weſt.

II. *A compariſon of the obſervations made at* Zurick *on the rain, and on the barometer, with the foregoing, during the ſame year**.

M. *Scheuchzer* compares his obſervations of the quantity of water, both in rain and melted ſnow, month by month, with what I have found at *Paris* at the obſervatory which I had ſent him by order of the *Abbé Bignon*, whereby we find, that it rained more at *Zurick* than at *Paris* in every month, excepting only the month of *July*. For he found in *Jan.* 34 lines $\frac{1}{2}$; in *Feb.* 109 lines; in *March*, 44 lines $\frac{3}{4}$; in *April*, 26 lines $\frac{1}{2}$; in *May*, 39 lines $\frac{3}{4}$; in *June*, 15 lines; in *July*, 38 lines $\frac{3}{4}$; in *Aug.* 66 lines; in *Sept.* 35 lines $\frac{1}{2}$; in *Oct.* 62 lines $\frac{3}{4}$; in *Nov.* 43 lines $\frac{1}{4}$; and in *Dec.* 25 lines.

This gave him for the whole year, 45 inches, 1 line, *Paris* meaſure; and he obſerves, that it is one of the greateſt heights of water which has been hitherto obſerved: but at *Paris* I found only 25 inches 2 lines, which, however, is one of the greateſt that has been ſeen here. I have related in the obſervations of another year, ſome reaſons which may ſhew, that there muſt fall more water and ſnow in the mountainous countries, than in the plains which are diſtant from them.

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* April 26, 1712.

He found the greateſt height of the barometer *Dec.* 22, at 27 inches; that day it was here at 28 inches, 3 lines, ¼, in the morning, with the common barometer; but with another at 28 inches, 6 lines, ½; the difference therefore was 18 lines, ½. The leaſt height of his barometer, was *Feb.* 9. at 25 inches, 11 lines, ½; and the ſame day here at 26 inches, 11 lines, ⅚, with the common barometer, and with the other at 27 inches, 3 lines, and the difference with this laſt is but 15 lines, ½; but thoſe days are not the ſame on which I here obſerved the greateſt and leaſt height of the barometer. Thus the difference between the greateſt and the leaſt height of the barometer, which I found at *Paris*, is 20 lines; and he finds it but 12 lines ½. This ſeems to me to ſhew, that the heights of the quickſilver in the barometers do not always come from the height of the whole atmoſphere, which cannot be very different in places upon the earth, which are not very far from each other; and at the ſame time, but from ſome particular accident of the air. However, if we took a mean difference of the height of the barometer at *Zurick* and at *Paris* in the obſervations which I have juſt related, we ſhould have almoſt 17 lines; and if we ſuppoſe 11 toiſes of elevation for a line of alteration in the height of the quickſilver, it would follow, that

Zurick would be 187 toiſes higher than *Paris* above the ſea.

He inlarges very much upon the meteors, and chiefly on the earthquake, which was felt at *Baſil*, of which M. *Bernoulli* has ſent him a very exact relation; and this is the reſult of it.

There were two ſhocks of this earthquake at but a little diſtance from each other, *Feb.* 9, between four and five in the morning; we felt one at *Paris*, *Oct.* 6, at eight in the evening, whence we ſee, that the cauſes of theſe earthquakes were very diſtant. At *Baſil* the earthquake was preceded by a very violent ſouth wind, like a tempeſt, or a hurricane, which was accompanied by an extraordinary heat in this ſeaſon of the year, tho' it was very cold before. At that time all the ſnow, which was very deep upon the ground, was melted in leſs than two hours, and all the rivers, and the *Rhine* itſelf, increaſed extraordinarily, which had not been ſeen till then; but when this wind ceaſed, the cold began again, and there fell a very great quantity of ſnow, to the height of 2 cubits. Theſe are his own words. M. *Scheuchzer* obſerves, that the ſame accidents of wind, and heat, were alſo obſerved at *Zurick*, but he does not ſay, the earthquake was felt there, he only adds, that at the ſame time he obſerved the height of the barometer to be 25 inches, 11 lines.

In the laſt place, he relates, that the fruits of the earth did not ripen well in his neighbourhood.

III. *On the ebbing and flowing of the ſea, by M.* Caſſini, *junior**; *tranſlated by Mr.* Chambers.

Men have long been enquiring the cauſe of that continual motion of the ſea, whereby its waters in the ſpace of 24 hours riſe, and advance, twice towards the ſhore, and fall again; or retire as often again therefrom.—The motion of the riſing water is called the flux, flowing, or flood, and its ſtate when arrived at its utmoſt height high water. On the contrary, the motion whereby it deſcends, is called the ebb, or reflux, and when it ceaſes to fall, low-water.

By this ſucceſſive change in the height of the ſea-water when retired, frequently proves too low in moſt parts of the ocean for veſſels of a certain burthen to enter or come out; ſeveral rocks, ſand banks, *&c.* are alſo covered at certain times by the ſea-water, which are left bare at others; ſo that it is of the utmoſt importance for the ſake of navigation, to be able to determine the periods of the flux and reflux, and to have an exact acquaintance with the ſeveral *phænomena* which obtain therein.

The memoir drawn up by the academy, and ſent by the count *de Pontchartrané* to the king's profeſſors of hydrography, in the ports of the ocean, has procured a great number of obſervations of the tide to be made with all the accuracy and ſucceſs imaginable.———An account has already been given of thoſe made at *Dunkirk* and *Havre*, to which we ſubjoin ſeveral obſervations, and drew new rules from them. The buſineſs now is to learn whether the rules which agree to thoſe two ports, be general and capable of being extended to the other ports.

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* April 6. 1712.

There is the more neceſſity for verifying theſe rules, as ſome of them go quite counter to what is commonly ſuppoſed, that the higheſt tides always happen about the days of the equinoxes. An opinion which ſeveral philoſophers have laid down as a certain rule, and ſtrove to give the phyſical reaſons thereof.

In order hereto, ſeveral more obſervations were required in other ports, like thoſe made at *Dunkirk* and *Havre*; wherein might likewiſe be expreſſed, the time of low-water, and the depth of the ſea in that ſtate; for as the times when the ſea is higheſt deſerve well to be known, as being the fitteſt for entring of ports; ſo thoſe when it is loweſt, are no leſs neceſſary to be known on account of their unfitneſſes for thoſe purpoſes. —— Something of this kind we have had occaſion to examine by a new journal of obſervations, both of high and low-water, made at *Breſt* by the ſieur *Montier de Longchamps*; it begins on the 10th of *June*, 1711, and ends on the laſt of *Jan.* 1712. The times of high-water for every day, both the morning and evening ones are here regularly noted with the height of water therein; and commonly alſo the time of the low-water, and the depth of water in that ſtate. To each of theſe obſervations care has been taken to affix the conſtitution of air, and the diſpoſition of the wind, which have a great ſhare in the accelerating, or retarding the tides, and the different heights obſerved therein.

By comparing the times of high-water in the new and full moons at *Breſt*, we find that the high-water obſerved to have come earlieſt was on the 26th of *September* 1711, at $3^h\ 13'\frac{1}{2}$ in the morning; and that the lateſt was on the 25th of *December* at $4^h\ 30'$ in the evening; ſo that there are inequalities in the time of the tides even in new and full moons, yet theſe inequalities may be in ſome meaſure reduced to a rule, by ſuppoſing the mean time of high-water at, *Breſt* to be at 3 45′, and making uſe of the rule preſcribed at *Dunkirk*, and *Havre*, which is to add 2′ to the mean time of high water, for every hour that the time of new or full moon anticipates the mean time of high water, and to ſubſtract on the contrary 2 for each hour, that the times of new or full moon comes behind the mean time of high-water.

For an inſtance, on the 28th of *Auguſt* 1711, the day of full moon, it was found high-water at $4^h\ 6'$ in the evening, and the time of full moon, as noted that day in the ephemeris was at $5^h\ 8'$ in the morning, the difference between $5^h\ 8'$ in the morning, and $3^h\ 45'$ in the evening; the mean time of high-water at *Breſt*, is $10^h\ 37'$; the double whereof 21 is the number of minutes to be added to $3^h 45'$, on account of the full moon's anticipating the mean time of high-water, the ſum gives the true time of high-water on the 28th of *Auguſt* 1711 at $4^h\ 6'$ in the evening, the ſame as was actually obſerved. So on the 9th of *December* 1711, the day of new moon, high-water was obſerved at 3^h $29'\frac{1}{2}$ in the evening, in the *ephemeris* new moon is noted for that day at $11^h\ 15'$ in the evening, the difference between $3^h\ 45'$, and $11^h\ 15'$, is $7^h\ 30'$, the double whereof 15 is the number of minutes to be ſubſtracted from $3^h\ 45$, on account of the times of full moons coming behind the mean time of high-water, the remainder gives the true time of high-water, on the 29th of *December* 1711 at $3^h\ 30'$ in the evening, within half a minute of what was actually obſerved.

As to the tides in the quadratures, high-water is then found at *Breſt*, nearly about the ſame time, tho'

tho' with inequalities ſomewhat greater than thoſe obſerved in the new and full moons, which is a neceſſary conſequence, ſuppoſing the motion of the tides to depend on that of the moon, ſince that planet is found by aſtronomers ſubject to greater inequalities in the quadratures than in conjunction or oppoſition.

Taking a medium between theſe inequalities, we ſhall find the mean time of high-water in the quadratures, at *Breſt* at 8h 57′, and the true time of high-water will be determined at *Breſt* for the days of quadrature, after the ſame manner as was done for the days of new and full moon, with this only difference, that in lieu of two minutes, we muſt add or ſubſtract 2½ from the mean time, for every hour the quadrature anticipates, or comes behind the mean time of high-water.

The mean time of high-water at *Breſt*, in the new and full moons, being fixed at 3h 45′, and in the quadratures at 8h 57′, gives us the retardation of the tides from the new and full moons to the quadratures 5h 12′, preciſely the ſame as was obſerved at *Dunkirk*, and within 2 minutes of what was determined at *Havre*; ſo that we may take it for a general rule, that the interval between the times of the tide from new and full moon to the quadratures, is leſs than from the quadratures to the new and full moons; and that the diurnal retardation of theſe tides goes in a regular kind of progreſſion, tho' the terms of this progreſſion do not fall upon the days of new and full moons, and quadratures, but one, two, or three days after; it being obſervable that the effect of the ſeveral phaſes of the moon upon the tides, is not communicated immediately; and that the higheſt tides uſually happen two days after new and full moons, as the loweſt uſually happen two days after the quadratures.

We have already obſerved that the higheſt tides do not always happen about the equinoxes, and all the loweſt about the ſolaſtices; but that the different heights of the tide, ſeem to have a nearer relation to the different diſtances of the moon from the earth; this rule, which agreed with the obſervations made at *Dunkirk* and *Havre*, and which we venture to extend beyond thoſe ports, is farther confirmed by the obſervations made at *Breſt*; for 'tis obſerved that in the high tides happening after new and full moon, the ſea riſes much higher when the moon is near the earth, than when ſhe is further diſtant therefrom; and that in two ſucceeding new moons, where the moon is at an equal diſtance from the earth, the tide uſually riſes to the ſame height.

For an inſtance, on the 10th of *November* 1711, the day of new moon, the moon's diſtance from the earth was 93600 of thoſe parts, whereof the mean diſtance is 100000, which is one of the ſmalleſt diſtances that can happen; and accordingly the tide roſe that day 19 feet 3 inches. On the 25th of *November* following, the day of full moon, the moon's diſtance from the earth was 106540 of the ſame parts, which is one of the greateſt diſtances that can happen; and accordingly the height of the tide that day was only found 16 feet, 9 inches, which is leſs by 2 feet 4 inches, than when the moon was nearer the earth: and this pretty much in the ſame proportion as the moon's diſtance from the earth. On the contrary, the diſtance of the moon from the earth being pretty equal on the full moon of the 29th of *July*, 1711, and the new moon enſuing on the 14th of *Auguſt*, the higheſt tides which followed on the

31 of *July*, and 16th of *Auguſt*, were found about the ſame pitch.

But the height of the tides, which in theſe obſervations ſeems to depend on the diſtance of the moon from the earth, does not ſuit ſo well with the rule of the equinoxes; for on the 26th of *September*, the day of full moon immediately following the autumnal equinox, high-water roſe in the evening to 17 feet, 5 inches; and on the day following, which was that of the higheſt tide, it roſe to 17 feet 6 inches; on the 12th of *October* next, the day of new moon, the height of the tide in the evening was found 19 feet, 5 inches; and the next day which was the time of higheſt tide, it roſe in the evening to 19 feet 6 inches; which was higher by 2 feet than on the 27 of *September*; tho' according to the rule of the equinoxes this tide ſhould have been lower than the former, which was nearer the autumnal equinox; but if regard be had to the moon's diſtance from the earth in theſe two obſervations, it will appear that the tide ſhould have been lower on the 27th of *September*, than on the 13th of *October*, ſince the moon's diſtance from the earth in the full moon of *September* was 103970 greater by much, than in the new moon of *October*, when it was 94680.

This relation of the tides with the diſtance of the moon from the earth, not only holds in the high tides ſucceding new and full moons, but alſo in the leſſer tides which follow the quadratures; for on the 6th of *Auguſt*, 1711, the day of the laſt quadrature of the moon, when her diſtance from the earth is 106300, which is one of the greateſt that can happen, it was found on the 8th of *Auguſt* following the day of loweſt tide, that the flood only roſe 10 feet 10 inches 4 lines, which is one of the loweſt that has been known.

On the 21ſt of *Auguſt* following, the day of the firſt quadrature, the moon's diſtance from the earth was 97700, which was much leſs than on the 8th of *Auguſt*, and accordingly on the 22d of *Auguſt*, the day of loweſt tide, it roſe to 12 foot 5 inches 6 lines, which was higher by 1 foot 7 inches than on the 8th of *Auguſt*. So on the 17th of *November* 1711, the day of the firſt quadrature, the moon's diſtance from the earth being nearly the ſame as on the 3d of *December*, in the ſame year the day of the laſt quadrature, the low tides immediately following thoſe phaſes were fonud about the ſame height.

It appears therefore beyond doubt, that the different diſtances of the moon from the earth is one of the chief cauſes of the different heights obſerved in the tides, which agrees likewiſe with Mr. *Childrey*'s obſervations, related in the *Philoſophical Tranſactions* for *October* 1670, where he gives an account of ſeveral high tides, which cauſed great inundations in *England*, and which all happened when the moon was *in perigeo.*

After laying down rules for determining the time of high-water, and the days of higheſt and loweſt tides, we proceed now to conſider what befalls in low-water, that is, after the ſea is withdrawn and ſunk to her loweſt ſtate, which is alſo obſerved twice a day.

It ſhould ſeem at firſt ſight, that the ebbing and flowing of the ſea being a ſucceſſive motion, the time of low-water ſhould be a medium between the times of high-water immediately preceding and following it; but by all obſervation the ſea employs more time in falling than in riſing; the reaſon may be, that the force which obliges the ſea to riſe ſtill ſubſiſts for ſome time after it has arrived at its utmoſt height, and thus keeping the water in ſome meaſure ſuſpended, prevents their falling with ſo much velocity, as they would do if nothing hindered their deſcent.

It follows hence, that to enter a port, 'tis by no means ſafe to come before the tide, ſince at equal diſtances of time the ſea is lower before, than after high-water.

But what is more remarkable in low-water is, that the higher the ſea had roſe, the lower it ſinks again; and the leſs high the tide had been, the leſs low will it be after the ebb.——For an inſtance, on the 13th of *Oct.* 1711, the day of the higheſt tide at *Breſt*, the water roſe 19 feet, 6 inches, above the fixed point; but upon the ſea's withdrawing, it was found 1 foot, 10 inches, below the ſome point; ſo that the ſea had fallen that day 21 feet, 4 inches.———On the contrary, on the 6th of *Sept.* the day of the loweſt tide at *Breſt*, high-water was found at 10 feet, 3 inches; and low-water, on the day preceding, was 5 feet, 11 inches; ſo that the ſea had only fallen that day 4 feet, 4 inches.

Hence it appears more eaſy to determine the time of high-water about new and full moons, than about the quadratures; ſince, in a ſpace of time almoſt equal, the ſea ſometimes riſes 5 times more in the new and full moons, than the quadratures.

From theſe obſervations it likewiſe appears, of how much importance it is to know the time of high-water in new and full moons, ſince on theſe occaſions the ſea-riſes or falls with more rapidity than about the quadratures; ſo that to bring a veſſel into a port where there is only depth of water ſufficient at the time of new and full moon, might be dangerous, if the pilot were not well informed of the preciſe time of high-water.

From theſe obſervations new rules might be laid down for determining the time of high and low-water, and the height thereof at *Breſt*, for all times of the year; and theſe rules being added to thoſe already preſcribed for finding the time and height of high-water at *Dunkirk*, and *Havre*, will include almoſt every thing to be deſired on this head, for the advantage of navigation.

Tho' our intention be not here to give a general theory of the ebbing and flowing of the ſea, yet we ſhall here ſubjoin our ſentiment as to the cauſe of the principal phœnomena obſerved therein.

In order to this, we ſuppoſe, that the ebbing and flowing of the ſea, may ariſe from the preſſion of the ſun and moon, upon the cœleſtial matter which ſurrounds the earth; but more from the preſſion of the moon which is nearer us, than that of the ſun, which is farther off.

Now in the new and full moons, when the ſun and moon are nearly in the ſame direction with regard to the earth, thoſe two luminaries poſſeſſing a place in the matter which ſurrounds the earth, neceſſarily compreſs that matter, and hereby preſs upon the water in the ſea, which being obliged to give way, and flow on either ſide from the place of preſſure to the diſtance of 90°, where the ſea muſt be at its greateſt height; in other ſituations of the moon from the ſun, the moon's action being directed differently from that of the ſun, the preſſure upon the earth muſt be leſs, and conſequently the ſea leſs high in its flux, as well as leſs low in its reflux; laſtly, the effect of the ſun being oppoſite to that of the moon in the quadratures, the preſſure occaſioned by the moon muſt be partly deſtroyed; and, conſequently, the ſea be higher in the time of low-water; but lower in the time of high-water, as is actually obſerved.

As to the different diſtances of the moon from the earth, they muſt have a ſenſible effect on the heights of the tides; for the preſſion of the moon upon the earth, muſt needs be greater when near the earth, than when further off. Since the motion communicated in a fluid near at hand, acts more forcibly than that communicated at a greater diſtance, that it may be obſerved in a fluid put into motion, whereof thoſe parts neareſt the place where the motion commences are more agitated than thoſe further off.

IV. *Reflections upon obſervations of the barometer made by M.* Vallerius *director of ſeveral copper mines in* Sweden, *by M.* de la Hire, *jun. tranſlated by Mr.* Chambers*.

Shewing M. *Vallerius*, who is a good mathematician, when he was ſome years ago at *Paris*, the changes which the mercury in the barometer undergoes between the top and bottom of the obſervatory, I deſired him to make the ſame experiments in the mines under his care, which he did accordingly laſt ſummer in the wells of *Flemengienus* and *Flemingſſchatet*, and the mines called *Falbunenſes* in the great copper mountain, and upon the mountain *Grufriis-barget* adjoining to thoſe mines the heavens being very cloudy, and the wind pretty ſtrong, ſo as to abate the heat.

He began his experiments with an obſervation of the barometer at the entrance of the mine, where he found the mercury at the 24th, 10th, and the fourth 100th of the *Swediſh* foot, which, in *Paris* meaſure, are equivalent to 26 inches, 9 lines, $\frac{52}{500}$ of a line——Here it may be proper to obſerve, that the *Swedes* divide their feet into 12 parts, each 10th into 10 more, which they call lines, and each line into 10 parts.

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* March 19 1712.

Then deſcending with the barometer into one of theſe mines 45 *Swediſh* fathom deep equivalent to 41 fathoms, 1 foot, 2 inches, 1 line, $\frac{1}{2}$, *Paris* meaſure, he found the mercury ſtanding at the 24th tenth, 7 lines, anſwering to 27 *Paris* inches, 1 line, and $\frac{26}{500}$ of a line; conſequently the mercury had aroſe 3 *Swediſh* lines for 45 of their fathoms deep, equal in to 3 lines $\frac{474}{500}$ of a line for 41 fathoms, 1 foot 2 inches, 1 line $\frac{1}{2}$ *Paris* meaſure.

He continued deſcending 45 *Swediſh* fathoms more, which is the loweſt he could go, and obſerving the barometer here, found the mercury at the 25ht tenth; ſo that it had aroſe 3 *Swediſh* lines, as in the former 45 fathoms, *viz.* to 27 inches, 5 lines *Paris* meaſure. Hence for 90 *Swediſh* fathoms we find 6 lines difference in the height of the mercury, which amount to 7 lines, $\frac{448}{500}$ for 82 fathoms, 2 feet, 4 inches, 3 lines, *Paris* meaſure.

But to be further aſſured of the accuracy of his obſervations, he made 2 in his return up again, whereby the whole depth was divided into 3 equal parts; whereas in going down, he had only made one in the middle; after aſcending 30 *Swediſh* fathoms, he found the mercury ſunk 2 *Swediſh* lines, which anſwers to 2 lines $\frac{316}{500}$, for 27 fathoms, 2 feet, 6 inches, 9 lines, *Paris* meaſure.———Aſcending 30 *Swediſh* fathoms further, he found the mercury ſunk 2 *Swediſh* lines. ———Aſcending 30 *Swediſh* fathoms further, he found the mercury ſunk 2 *Swediſh* lines.—— And laſtly, arriving at the entrance of the mines, which is 30 fathom further, he found the mercury ſunk 2 *Swediſh* lines lower, ſo that it now ſtood at the 24th tenth, and 4 lines, as at firſt.

Not contented with theſe obſervations, M. *Vallerius* made others on the mountain *Grufriis-berget*, adjoyning to the mine juſt mentioned, and aſcending the mountain 15 *Swediſh* fathoms perpendicularly high, he found the mercury 1 *Swediſh* line lower than at the foot of the mountain, or the entrance of the mine, which amounts to 1 line, $\frac{158}{500}$, for 13 fathoms, 4 feet, 3 inches, 4 lines $\frac{1}{2}$ *Paris* meaſure———Continuing to aſcend 15 *Swediſh* fathoms higher, he found the mercury 1 *Swediſh* line lower than in the former obſervation——and arriving at length at the top of the mountain, which was 22 *Swediſh* fathoms higher than in the former obſervation, and conſequently 52 *Swediſh* fathoms above the entrance of the mine, he found the mercury ſunk 1 *Swediſh* line $\frac{2}{10}$, ſo that it now ſtood at the 24th decad, and $\frac{8}{10}$ of a line, and thus for 52 *Swediſh* fathoms had ſunk 3 lines $\frac{2}{10}$, which in *Paris* meaſure makes 4 lines $\frac{166}{500}$, for 47 fathom 3 feet, 2 inches, 10 lines $\frac{8}{10}$.

Deſcending the mountain again, he obſerved the heights of the mercury in the ſame places as before, and found the ſame differences, whence he infers, that 9 lines, and $\frac{2}{10}$ of mercury, correſpond to 142 fathoms of air, *Swediſh* meaſure, which amounts to 12 lines $\frac{54}{500}$, for 129 fathoms, 4 feet, 11 inches, 1 line, and $\frac{450}{500}$.

For the more exactneſs of his obſervations, M. *Vallerius* informs me that he made them with two barometers which perfectly agreed from beginning to ending.

By examining the obſervations above related, that from the bottom of the mine, to 27 fathoms, 2 feet, 6 inches 9 lines perpendicular height, above the top of it, there are 109 fathoms, 4 feet, 3 inches, for which the mercury ſunk 10 lines, and $\frac{264}{500}$, ſo that there was always a line difference in the height of the mercury, to 10 fathoms, 1 foot, 6 inches, 4 lines, the mercury at the bottom of the mine ſtanding at 27 inches, 5 lines, and at the top of the mountain 109 fathoms, at 26 inches, 6 lines, and $\frac{236}{500}$.

Theſe I take to be the firſt experiments of the barometer, in places of this depth, which makes them of the more conſequence, as they ſhew that the ſame differences in the height of the mercury, correſpond to the ſame height of air, whether it be upon a mountain, or within the ground, even in a deep mine, where one might imagine that the great plenty of vapours, muſt have made the air conſiderably heavier than elſewhere.

Now upon comparing the obſervations of M. *Vallerius*, with thoſe made in *France*, it appears that a line difference in the height of mercury in *Sweden*, correſponds to a leſs height of air, than the ſame is found to do by Meſſ. *Caſſini*, *Picard*, and *de la Hire* in this country.

For M. *Caſſini* at the foot of the mountain *Notre-Dame de la Garde*, near *Toulon*, found his barometer at 28 inches, which after aſcending to the top, 178 fathoms 2 feet, was ſunk to 26 inches 8 lines, ſo that 178 fathom 2 feet, gave a diminution of 16 lines, which is 10 fathoms 5 feet per line, ſuppoſing the air equally denſe in this whole height——M. *Picard* again on *Mount St. Michael* found his barometer ſink from the *Greve*, to the dial on the middle of the church, 4 lines ½, the depth between which places is 64 fathoms, whence he infers, that a line difference in the height of mercury anſwers to 14 fathoms, 1 foot, 4 inches.——Laſtly, my father on the mount *Clairet*, by *Toulon*, found his barometer at 26 inches, 4 lines, $\frac{1}{2}$; but deſcending to the banks of the ſea he found it at 28 inches, 2 lines, which is 21 lines $\frac{1}{2}$ for 257 fathoms depth, or 12 fathoms per line; and from the like experiments at *Meudon*, he found 6 lines, $\frac{3}{4}$, ſinking of mercury, for 85 fathoms, 2 feet, height of air, which gives 12 fathoms, and 4 feet, nearly for a line; by other experiments made at the obſervatory, he found a line of mercury anſwer to 12 fathoms, 2 feet and $\frac{2}{3}$.

M. *Caſſini*, jun. in the memoirs for the year 1705*, gives a table of the heights of air correſponding to the heights of mercury in the barometer, founded on a rule eſtabliſhed by M. *Maraldi*, wherein it appears, that at 27 inches, 5 lines, the height of mercury found by M. *Vallerius* at the bottom of his mine, 11 fathoms, 1 foot, correſpond to a line difference of height of mercury; and that at 26 inches, 6 lines, which is nearly the height M. *Vallerius* was at on the mountain *Grufriisberget*, 13 fathoms of air anſwer to a line of mercury.

The above-mentioned heights correſponding to a line of mercury are all greater than that found by M. *Vallerius*, tho' they ſhould be ſmaller, ſuppoſing as we may probably do, that the air is leſs and leſs denſe the further it is off the earth, ſince they are all founded on experiments which began at a point where the barometer ſtood at 28 inches; whereas that of M. *Vallerius*, when loweſt, was only at 27 inches, 5 lines.———But as this proof may not ſeem convincing, I ſhall ſubjoin another founded on the ſame principle, *viz.* that the air is an heavy elaſtic body.

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If the atmoſphere * C X, which I ſuppoſe of a certain extent from the centre of the earth C, be divided from the top ſo as to give all the points X, U, T, S, *&c.* where the mercury changes a line in its height, theſe points will mark ſpaces which go continually diminiſhing in a certain proportion.

If now a barometer be placed in any part of the atmoſphere, thus divided as in R, and the mercury be found for inſtance at 26 inches; if we would have it to ſink a line, we muſt raiſe it to S; but if while the barometer ſtands in R, the atmoſphere growing higher, makes the mercury riſe 27 inches. To make the mercury now fall a line, we muſt not raiſe it the height RS, but the height EF; for that the different changes of the atmoſphere, produce the ſame effects on a barometer which keeps it place, as would be produced by removing it into a higher, or lower place, while the air undergoes no alteration.

But by the experiments of M. *Vallerius* we find, that at a certain height of the barometer, a ſmaller height of air anſwers to a line of mercury in *Sweeden* than in *France*, than is found at a greater height of the barometer in *France*, which ſhews, that the atmoſphere is much higher in the northern countries than 'tis here; and hence it ſeems to follow, that the perpendicular height correſponding to a line of mercury at a greater height of the barometer in *France*.

It had been conjectured from the obſervation of M. *Richer*, at *Cayenne*, that the atmoſphere becomes higher as it is further from the line, the greateſt height of mercury which he found there during a whole year, being only 27 inches, 1 line; whereas, at the obſervatory, no year paſſes wherein it does not riſe above 28 inches.

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* Plate V. Fig. 12.

But the atmoſphere being higher in the northern than the ſouthern countries, the refraction muſt be much greater, which we find accordingly from the obſervations of ſome aſtronomers who attended king *Charles* II. of *Sweden*, in an expedition towards the north pole.

It remains to account for the differences between the obſervations made in theſe countries, for determining the perpendicular height of air correſponding to a line of mercury.——After ſifting the ſeveral changes which may befall the air, I apprehend this difference owing to two principal cauſes; the firſt, to a greater or leſs height of the atmoſphere; and the ſecond, to trains of vapours diffuſed thro' the air near the earth, which on ſome occaſions may make the ſpaces of the air: they are in heavier without any conſiderable alteration in the height of the whole atmoſphere.

V. *A continuation of the obſervations on the bezoars, by M.* Geoffroy, *junior*[*].

In my firſt obſervations I have remarked, that there is generally in the centre of each bezoar ſome foreign body, about which the bezoardic *ſtrata* are formed and diſpoſed. It has alſo appeared to me, that it might be a ſign, that the ſtones are not counterfeited, as thoſe who ſhould endeavour to counterfeit them, would not think of uſing a pre-

[*] July 6, 1712.

precaution, which would be of no ſervice to them; and beſides, they would never take the pains to ſeek for ſo great a variety of ſubſtances, as thoſe which ſerve for a baſe to the different bezoar ſtones.

The very foſſil bezoars are formed after the ſame manner. *Boccone* has obſerved kernels in them of different ſorts, pebbles, gravel, wood, metal, coals, *&c.* I have examined that ſort, called *Priapolites*, which grows in *Languedoc*; and one of them was given me by M. *Bon*; the centre of which is occupied by a piece of rock cryſtal.

Among the different kernels found in the animal bezoar ſtones, I have obſerved one which appeared to me to reſemble the ſtones of *Caſſia* or *Tamarind*, but ſmaller. I have, however, found ſince, that it might be the fruit of a pod, which I had not ſeen before, reſembling that of the pod of the tree, called, *Acacia vera Ægyptiana*. This tree grows in *Egypt*, *Arabia*, and other places. The pod, which is brought to us from *Senegal*, is 3, or 3 ½ inches long, and 9 or 10 lines broad; it is compoſed of an outer and an inner membrane. The outer membrane is very tender, of a brown colour, and faſtened to the inner one, which is cartilaginous, and very thin. The matter, which unites them, is gummy, of a tranſparent yellowiſh colour; it melts in the mouth, and has a very rough taſte. In the longeſt pods I have found 8 grains ſeparated from one another by a ſort of contraction, which reunites the ſides of the membrance. Each cavity of theſe pods contains a flat grain, reſembling a lupine, ſometimes exactly circular, and ſometimes a little compreſſed by the contraction of the pod, which is cloſer in the middle than at the two ends; ſo that the fruits of the middle of the pod are a little compreſſed, and thoſe of the end are exactly round.

What made me judge that theſe fruits were thoſe which I had obſerved in the bezoar, which is round, and a little flatted, is that I have found them to have the ſame marks, and among many others, a circular whitiſh line, drawn upon each face of the fruit, ſuch as appears upon that which is found incloſed in the bezoar. I put ſome of theſe fruits in water, they ſwelled almoſt in the ſame manner as they would have done when found in the ſtomach of the animal, where they began to be covered with the bezoartic matter. The tincture which I drew from theſe fruits was red and very ſour. I threw a little vitriol into it, and it grew black: theſe ſeeds, and their pods are uſed in the country where they grow, to tan leather. From their decoction in water is drawn a juice, which is thickened, and brought to us under the name of *Succus Acaciæ*. It is alſo pretended, that from this *Acacia* tree, that the gum is obtained, which we call gum arabic, and gum ſenegal. Is there any probability that the makers of the bezoar ſhould go to look among other things for the fruit of the *Acacia*, to make one of the baſes of their compoſition? And is it not more likely that theſe fruits and ſome others, which ſerve for nouriſhment to the animals, cauſe by their aſtriction a thickening of liquors in the ſtomachs of thoſe animals, which eat the moſt of it; this thickening of liquor may cauſe the formation of the bezoar ſtones.

This is the manner in which thoſe ſtones grow in the ſtomach of the animal that bears them, and grow to the ſtate in which we find them. There may be ſeveral found in the ventricle of a ſingle animal. *Tavernier* ſays expreſly, that ſix of theſe goats which he had for a preſent, had in all 17 bezoars, that they might be felt on the outſide and counted, which increaſed the price of theſe animals, in proportion to the number of bezoars that were felt in them. This agrees perfectly with what *Cluſius* relates of the animals which afford the occidental bezoar. He ſays, that a friend of his in *Peru*, who had firſt made the diſcovery of the occidental bezoar, being deſirous to know how theſe ſtones were formed in the body of theſe animals, diſſected one of them, and found in the ſtomach a ſort of pouch, where theſe ſtones were ranged in order, as the buttons upon a coat.

Theſe two paſſages are quite oppoſite to what *Pomet* tells us, that only one bezoar is found in the belly of each animal. He alſo aſſures us, that he would not have ventured to contradict the authors, who have treated of it; had he not had a piece in his hands to juſtify his opinion.

It will be proper to examine it here, as nobody, that I know of, has publickly expoſed *Pomet's* error concerning the pretended coat of the animal *bezoar*, which he ſaid was one of the greateſt curioſities that had been ſeen for a long time in *France* in the opinion of all judicious perſons.

This coat is, ſays he*, *of the bigneſs of a gooſe egg, having on the outſide a ſhort rough hair, of a tawny colour, which being cut in two, diſcovers a thin brown ſhell, which covers another white ſhell, as hard as a bone, in which the ſtone is contained, that is called the* bezoar.

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* *Pomet in his traité des drogues, livre des anim.* pag. 10.

Now this ſo ſingular a covering of the *bezoar*, of which he pretended to have made the diſcovery, is not any part of the animal which bears the *bezoar*, but an exotic fruit, in which either *Pomet*, or ſome quack, whom he had ſuffered to impoſe upon him, had very artfully incloſed a *bezoar* ſtone. This fraud was not diſcovered 'till about a year ago. As I was to examine this ſingular drug of M. *Pomet*'s in company with M. *Vaillant* and M. *de Juſſieu*, demonſtrators of the plants in the royal garden, we perceived that this pretended covering could not be a part of any animal, and that it muſt be ſome fruit but little known: This was afterwards verified by M. *Vaillant*, who found he had ſome of theſe fruits, and could make *bezoars* of them, with their coverings, juſt like the *bezoars* ſo much eſteemed by *Pomet*; and I have made ſome of them myſelf. This fruit grows upon a ſort of palm-tree deſcribed by *John Bauhinus*, and called by him *Palma cuciofera:* this fruit is alſo deſcribed by *Theophraſtus*. The tree grows in *Egypt*, *Nubia* and *Ethiopia*. *Cordus* calls it *Nux Indica minor*, and has given a deſcription of the fruit, ſuch as I have juſt now related from *Pomet*, in ſpeaking of the coat of the *bezoar*. This deſcription wants only one particularity omitted by *Pomet*, which is the ſkin that again covers the whole fruit, and is of a tawny yellow colour; the fruit has a footſtalk divided into 6 parts, 3 great ones, and 3 ſmall ones. This would have been ſufficient to have undeceived him, or thoſe who have been deceived after him, and it is of ſervice toward the perfection of natural hiſtory, that frauds of this kind ſhould be carefully revealed.

It is not without reaſon that I put in my laſt memoir in the rank of *bezoars* all the ſubſtances that are formed in *ſtrata* in the bodies of animals. The pearls which I reckoned in this number, deſerve it ſo much the better, as I have found ſome in certain ſhells, ſo like the common *bezoar*, that they are hard to be diſtinguiſhed at firſt ſight: Theſe pearls are engendered in a ſort of ſhell-fiſh, called *Pinna Marina*, *Pinna ſive Aſtura Mathioli*; we ſee a great quantity of them on the coaſts of *Provence*, where they fiſh in *April* and *May*; this ſort of fiſh is called *nacre* in that country.

The pearls which are found in theſe ſhells, are all of the ſame water; ſome are, as I have ſaid, perfectly like *bezoar* ſtones; others of a coral, and amber colour, and others of a pearl colour, but more leaden; the moſt uſual ſhape of them is that of a pear. All theſe varieties of ſhape and colour do not hinder them from being of the ſame nature, ſince they grow in the body of the ſame fiſh; I have four of a different water and ſhape, which were taken out of the ſame ſhell. That theſe, and all other pearls are formed in the body of ſhell-fiſhes, as the common *bezoar* is in the goats that furniſh it, is not difficult to prove, ſince upon breaking them they are found to have ſtripes like certain *bezoars* already mentioned, and formed about a *nucleus* or kernel, which ſeems itſelf to be a little pearl.

Some of them are ſo uneven, that they do not preſerve the figure of pearls, but the matter of them is always diſpoſed in *ſtrata*, like the bezoars. Now it is never queſtioned but that the oriental pearls are of the ſame nature with thoſe which grow in other ſhell-fiſhes, as in the common oyſters, and in the different ſorts of muſcles. All the difference between them comes only from their different water, but is every where the ſame matter, and the ſame ſtructure, as may plainly be ſeen in the different pearls found in the *pinna marina*. Pearls therefore muſt be looked upon as true *bezoars*, as to their nature, tho' they are not perfectly ſuch as to their virtue.

The pearls are not the only thing that is obſervable in the *pinna marina*. This ſhell-fiſh, which is a ſort of great muſcle, conſiſts of 2 large pieces, rounded at top, and very much pointed at bottom, very unequal on the outſide, brown and ſmooth on the inſide, towards the point inclining to the colour of mother of pearl. They are of different ſizes from 1 foot to 2½ in length, being in the broadeſt part about ⅓ of their length, theſe ſhells are ſo thin, that they are tranſparent; what is moſt remarkable in them, is a ſort of tuft, about 6 inches long, more or leſs according to the ſize of the ſhell. This tuft is ſituated toward the point of the ſide oppoſite to the hinge; it is compoſed of ſeveral filaments of a very fine brown ſilk, theſe little threads being viewed thro' a microſcope appear hollow, when burnt they afford an urinous ſmell like ſilk. The ancients have called this ſubſtance *byſſus*, either on account of its reſemblance to the *byſſus*, of which they wove precious ſtuffs, or perhaps it might be the very *byſſus* itſelf, which they uſed for that purpoſe; for the moſt able criticks have not ſufficiently cleared up what we are to underſtand by the *byſſus* of the ancients; they have only diſtinguiſhed two ſorts of it, that of *Greece*, which was found only in the province of *Elis*, and that of *Judea*, which was the fineſt. The ſcripture *informs us, that this was uſed in the ſacerdotal ornaments, and the wicked rich man was cloathed with it. But as the ancients under the name of *byſſus*, have confounded cottons, and all other ſtuffs that were more precious than woollen, it is not eaſy to ſay exactly what their *byſſus* was, and whether it was obtained only from the ſhells juſt mentioned; this is certain, that *Ariſtotle*, who calls the ſilk of theſe ſhells *byſſus*, ſays it may be wowen, and therefore it can hardly be queſtioned but that it was uſed for the cloaths of great perſons, in the ages when ſilk was but little known, and rarely ſeen; in ſhort this *byſſus*, tho' coarſely ſpun, appears much finer than wool, and approaches pretty near to ſilk: they now make ſtockings of it, and other works, which would be more valuable, if ſilk was leſs common: to ſpin this ſort of *byſſus*, they leave it in a cellar, to grow moiſt and ſoft; and then they comb it, to ſeparate the flocks and impurities that ſtick to it, after which they ſpin it, as they do ſilk.

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* In our tranſlation of the bible *byſſus* is rendered *fine linnen*.

The fiſhes, which afford the *byſſus*, make uſe of it to faſten their ſhells to the neighbouring bodies, for as they are planted directly upon the point of their ſhells, they have need of theſe filaments, which they extend all around, like the cordage of a maſt, to keep themſelves in that ſituation.

It is probable that the *pinna marina* forms theſe ſorts of threads, with the ſame mechaniſm that M. *de Reaumur* has obſerved in the ſea-muſcle; but thoſe of the *pinna* are more fine and ſilky; and according to *Rondeletius*, they are as different from the threads of the muſcles, as ſilk is from hemp, which may be ſeen by comparing them together.

There are ſome little crabs that get into the ſhells of the *pinna*, of which the antients have related ſuch ſingular facts, that it may not be amiſs to examine them here.

They thought this little animal grew with the fiſh of the *pinna*, for its preſervation, and therefore called it the *guardian of the pinna*, imagining that the fiſh periſhed as ſoon as it come to loſe its guardian. And this is what they thought made the little crab ſo uſeful to its hoſt.

As the *pinna* is without eyes, and beſides is not endued with a very exquiſite ſenſation, whilſt its ſhells are open, and the little fiſhes enter, the crab gives it notice by a ſlight motion, that by contracting its ſhells at once, the fiſhes may be taken, and then the *pinna* and the crab divide the booty between them. Thoſe who did not believe that the crab received its birth in the ſhells of the *pinna*, raiſe the wiſdom of this little animal ſtill higher, which in order to lodge itſelf in the ſhells of fiſhes, takes its opportunity when they are open, and has the cunning to put in a little ſtone to hinder them from ſhutting, and ſo eats up the fiſh. But all theſe circumſtances are like a great many others related by the ancient naturaliſts, without much foundation; and this is what has contributed to decry their works, tho' they otherwiſe inform us of ſeveral things that are true and curious. What they tells us here of the little crabs that lodge between the ſhells of the *pinna*, is eaſily diſproved; for in the firſt place, theſe little animals are found indifferently in all bivalve ſhells, as oyſters and muſcles, as well as in thoſe of the *pinna*, where we alſo ſometimes find ſome little ſhell-fiſhes, which get in or faſten themſelves upon them. I have a little *concha venerea*, which was found ſhut up and alive in the ſhell of a *pinna*. Beſides the fiſh of theſe ſhells does not live upon fleſh, any more than the muſcles and oyſters, but only upon water and mud. Thus the cunning of the little crab is of no uſe to it.

Laſtly, the little crabs do not eat the fiſhes of the ſhells where they lodge, for we find theſe fiſhes ſound and whole with the little crabs that accompany them.

It is only chance therefore that throws theſe little animals into the ſhells whilſt they are open, or elſe they retire thither for ſhelter, as they are very often found in the holes of ſponges and ſtones, and in the exterior cavities of the ſhells.

As I have referred to the ſecond claſs of bezoars the ſtones of the ſame nature which are taken from animals, I ſhall add thoſe which I lately obſerved in the bags of the beaver, which are called *caſtor*. Among ſeveral that I opened, I found one which ſeemed bigger than the reſt, and was filled with ſtones of different ſizes. According to the common prejudice, I ſhould have thought that theſe bags had been falſified and filled with ſtones to increaſe their weight; but upon examining them, I perceived that all theſe ſtones adhered, and were pretty regular in their ſhape. I preſented ſome of theſe ſtones to the flame of a candle, and they burned like thoſe taken from the gall-bladder, and had a ſmell of caſtor. Theſe ſtones pretty much reſemble the kernels of medlars, as thoſe commonly do which are found in the gall-bladder. They are tender, and diſpoſed in *ſtrata*, which are ſeparated by membranes diſperſed through the ſubſtance of the bag, and forming the partitions of the cells. The biggeſt that I have found are 6 lines long, and 4 broad, and 3 in thickneſs. The others, which are in greater number, diminiſh in bigneſs, and the ſmalleſt are but about the ſize of pins heads. It is not probable that theſe ſtones have been added in the caſtor, conſidering the manner in which I have already obſerved them to be conſtructed.

The juice therefore contained in theſe bags muſt have thickened and curdled, about the membranes, or their glands; and ſo have ſerved for a baſe to the formation of theſe ſtones. It is obſerved, that ſtones are formed in all the cavities of animal bodies, and even in the glands; and thus the name of bezoar becomes ſo extenſive. I think therefore we may range theſe ſtones in the number of bezoars, as well as the different ſorts of pearls, ſince they reſemble the bezoar in their ſtructure and vertue. The caſtor being uſed in medicine to fortify the brain, reſiſt poiſon, aſſuage the vapours, and drive them out by perſpiration, the ſtones which are found to contain the ſame principles, muſt have the ſame effects, and conſequently the ſame virtues as the bezoardic ſubſtances. As I treat of the caſtor only with regard to the ſtones, which I have obſerved, I ſhall not ſtop here to deſcribe the animal, nor the bags which contain the ſubſtance called caſtor, ſeeing the anatomy of it has been already made by the academy.

I ſhall only propoſe my opinion on the choice of this ſubſtance. I agree with thoſe who are acquainted with it, that ſome of it may be falſified, but I believe that the difference in its ſmell and conſiſtence comes rather from the climate, the food, and the age of the beaver, than from any counterfeiting. The moſt common, and leaſt eſteemed caſtor is that from *Canada.* It is looked upon as counterfeited, becauſe it has either no ſmell at all, or a diſagreeable one. I have opened ſeveral that were ſoft, and of very little ſmell, and yet without any appearance of ſophiſtication, for the cells were neither ſwoln nor torn. On the contrary they were divided by membranes adhering to the covering, as we obſerve in thoſe which are not ſuſpected to be ſpurious. The caſtor of *Dantzick* is eſteemed the beſt, and yet that of the *Levant* ſurpaſſes it.

There are ſome beavers alſo found in *France*, in ſome parts of the *Rhone*; they dry their bags, and this ſort is very good. I have ſome in my collection, dried by an apothecary of *Villeneuve-lès-Avignon*, which are very good and large without being adulterated. I have found that this caſtor was not at all inferior to that of *Dantzick*. That of the *Rhone* is commonly ſold for that of *Dantzick*, there being no other difference between them, but in that of *Dantzick* having the ſtrongeſt ſmell. I am perſuaded, that our caſtor of the *Rhone*, has the ſame quality with that of the *Levant* and of *Dantzick*; the bags are dried in the chimney where the liquor may ferment as it dries, which cauſes the caſtor to acquire continually a ſtronger and more proper ſmell.

VI. *A machine to diſengage the horſes abſolutely, and at once from a coach, when they are headſtrong and run away; by M.* de la Hire, *the ſon**.

The accidents, which happen when horſes run away, are ſo great, that I thought I ſhould do a good piece of ſervice to the publick, in finding out ſome eaſy way of hindering theſe accidents.

Among all the machines I have thought of for this purpoſe, I have found none more ſimple than that I am going to deſcribe; after having explained the figure which repreſents the whole fore-part of a coach, called the *fore-carriage*, in order to ſhew what is called the *ſplinter-bar*, with all the parts that depend upon it, the place where it is faſtened, and its uſe.

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* *Nov.* 16, 1712

**Fig* 13. AB is the pole, of which the fartheſt extremity from the coach, which is furniſhed with a hook QR, is repreſented underneath.

CD, CD the two futchells.

EF the ſplinter-bar faſtened upon the two futchells, with 2 iron ſcrews.

NN are the cramps of the ſplinter-bar, which go into it, and are faſtened at the mouldings, they hinder the 2 leather rings GH, GH, which paſs within as in the 2 cramps PP of the bars, from getting out at the end of the ſplinter-bar.

LK, LK are the two bars ſuſpended, as we ſee, at the two ends of the ſplinter-bar, by means of the two leather rings, and in the middle of the bars are the cramps PP, which hinder them from ſlipping in the leather rings.

LM are the traces with which the horſes draw the coach; they are fitted at each end of the bars in ſuch a manner, as to embrace them the more ſtrongly, in proportion as the horſes draw with more effort.

S, the foot-board.

Fig. 14. repreſents the end of a ſplinter-bar more large, with the bar which is faſtened to it by the leather ring, to ſhew all the ſmall parts of it better.

I believe this explanation of the fore-carriage will be ſufficient to underſtand, what I ſhall ſay in the ſequel of this memoir.

The machine in queſtion is applied at each extremity of the ſplinter-bar, which is furniſhed with 2 mouldings, two or three inches diſtant from each other, and raiſed about ½ an inch above the body of the ſplinter-bar, which is cylindrical, beween theſe two mouldings.

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* Plate V.

It is between theſe two mouldings that the leather ring of the bar is placed; it is made of ſeveral leather thongs ſewed together, the two ends of which are joined by a buckle; in this manner the leather ring is drawn into length, and at one of its extremities embraces the end of the ſplinter-bar, and the other at the middle of the bar.

Fig. 15. One of the ends of the ſplinter-bar is repreſented by the letters A A; of the two mouldings, which are there, that which is moſt at the extremity of the ſplinter-bar has been ſhaped to 8 angles, that the end of the ſplinter-bar may be the leſs diminiſhed, becauſe of the ſubjection which I am going to mention; there are 4 of them large and 4 ſmall, to give a greater ſeat to the ſides of the ferril, which ſhall be fitted to it; the figure that has been given to this moulding muſt be inſcribed in the end of the ſplinter-bar, which bears the leather rings of the bar, that it may enter into the iron ring O.

This moulding being thus made octangular, has two of its great ſides placed vertically, one towards the coach, and the other towards the horſes; the two others are placed horizontally, one above and the other below; it is furniſhed with a ſort of iron ferril B, which is alſo octangular.

From the face of this ferril, which is toward the coach, there comes out perpendicularly an iron pin D, about an inch and $\frac{1}{2}$ long. The extremity of this pin D is rounded like a pivot, that it may enter into a round hole at the extremity L, of the piece of iron LK, which I call the traverſe, and be rivetted in ſuch a manner into it, that this traverſe, which is perpendicular to the pin D, may move circularly in it.

The traverſe LK about 4 inches long, and 10 lines broad in the place where it is moſt ſo, has toward the extremity K a hole M, which pierces thro', to let the bit of iron paſs when it is preſſed upon it.

The piece of iron F, which is an inch and $\frac{1}{3}$ high, is raiſed perpendicularly upon a pin E, which being parallel to that which is mark'd D, riſes from the middle of one of the vertical faces of the ſort of ferril C, namely from that which is toward the coach; the moulding to which this ferril belongs, has been made quadrangular; ſo that two of its faces are vertical, and two horizontal, and the ferril C is circumſcribed to the part of the ſplinter-bar, between the 2 mouldings.

Acroſs the two upper horizontal faces B and C of the two ferrils, as well as of the oppoſite faces, and acroſs the ſplinter-bar, paſs two ſkrews, which have their heads marked 3 and 4, and enter into the lower horizontal faces, which are oppoſite to B and C, to fix the two ferrils B and C to the ſplinter-bar AA.

The thong PQRSTVXY is the leather ring of the bar, it embraces at one of its ends that part of the ſplinter-bar, which is between the two ferrils B and C, becauſe the extremity PQ is ſewed to the under part; it is ſlit in the middle of the ſpace between the two ferrils, to receive in its thickneſs the iron ring O; this ring embraces the ſplinter-bar in this place, and has a pin or tongue 8, which is perpendicular to it, and enters into the hole Z, marked toward the extremity XY of the leather ring of the bar.

When the tongue 8 is entered into the hole Z of the leather ring, if you paſs the bar into the leather ring at the place TV, you will make the ring O turn, and the tongue 8 will come before;

it is to hinder it from coming before, that we ſhall lower the traverſe LK upon the pin E, and make the double key H enter into the hole G, which goes thro' the piece F; but as the dirt might fall between the plates of the key H, and hinder it from being drawn back, there is faſtened upon the traverſe LK the box N, the entrance of which will be ſhut by the head of the key when it is put in, ſo that nothing can get into the box.

We ſee by the conſtruction of the machine, that the horſes which draw the bars, make a continual effort to raiſe with the tongue 8 the traverſe LK, which cannot get looſe from the piece F, becauſe of the key H which paſſes through it.

The other bar is faſtened in like manner, at the other end of the ſplinter-bar.

There remains nothing now but to ſhew how we may draw all at once, in the ſame inſtant, the key H, and that which is at the other end of the ſplinter-bar.

To do this we take a leather ſtrap or thong ſlit in one part of its length, one of the ends of which paſſes over one pulley, and the other over another; theſe two pulleys are placed horizontally, and are in a double block, the gudgeon of which enters perpendicularly into the middle of the length of the ſplinter-bar, and into the face which is toward the coach.

The two ends I of this ſtrap, after having paſſed over the pulleys, embrace the rings 6, which are behind the heads of the keys, and there theſe two ends, or theſe two parts of the ſlit end of the ſtrap are ſewed together; the other end of the ſtrap, which has not been ſlit, paſſes under the foot-board and thro' the axle-tree of the little wheels, and is faſtened to a ſilk cord, which enters the coach at a hole made in the middle of the fore-part, like that thro' which the ſtring paſſes, to give the coachman notice when to ſtop.

It is eaſy to ſee, that if the perſon in the coach pulls the ſtring, whilſt the coach is going on, he will diſengage the pins H from the holes G, that at the ſame time the tongues P will raiſe the traverſes LK; that the end of the leather ring XY will come off from the tongue 8, and conſequently that the bars will be no longer faſtened to the ſplinter-bar; and as the bars are no longer faſtened, the coach can no longer be drawn by the horſes, becauſe they are only faſtened by this part; having ſubſtituted a ſpring inſtead of the little ſtrap, to hold the great leather rings engaged in the iron hook, placed at the end of the pole; theſe leather rings lower this ſpring, when the horſes continue to go on, and the bars are no longer faſtened to the ſplinter-bar, which I ſhall explain more at length, when I ſpeak of the other advantages of this ſpring.

The convenience of this machine is, that it may very eaſily be taken off from one coach and put upon another; for you need only undo the two ſkrews 3 and 4, and to unſew and ſew again the two ſtraps which hold to the two keys, becauſe I ſuppoſe that care will be taken, before the two mouldings at the two ends of the ſplinter-bar are made angular, as I have obſerved, to faſten a double pulley to the middle of the ſplinter-bar, and to make a hole thro' the axle-tree of the little wheels of all the coaches, to which we would have the machine applied, and they will ſerve as uſual, when the machine is not on, becauſe we may paſs into the holes of the ſplinter-bar, where the two ſkrews 3 and 4 enter, the pins of a cramp, which ſhall be furrowed at the end, to ſkrew nuts upon them to hold the cramp faſt to the ſplinter-bar, and by this means hinder the leather rings of the bar, from ſlipping off the end of the ſplinter-bar, as is done to all coaches.

It is not enough to ſhew the way to prevent accidents, when two horſes run away, but we muſt alſo prevent them, when there are 4, with a poſtillion or without one, and when there are 6, and when the poſtillion has been thrown under the horſe that he rode upon, or when he cannot govern the horſes.

When there are four horſes to a coach, the two before are faſtened either with traces, to the wheel horſes, or to two bars faſtened to a ſplinter-bar by two leather rings, like thoſe next the coach; this ſplinter-bar, to which the two fore-horſes are faſtened, is encompaſſed in the middle part by a leather ring, in which their paſſes another, where the end of the pole enters, which has a hole to receive an iron pin, which is faſtened to the end of the hook, with a little ſlip of leather, and againſt this pin the leather ring reſts into which the pole enters; in this mannner the fore-horſes draw the coach by the end of the pole, but when inſtead of four horſes they put 6, they only faſten the two firſt, to the two ſecond with traces; and ſo the four firſt are faſtened to the coach only by the end of the pole.

In order therefore to hinder the accidents which might happen, if four or ſix horſes were to run away, we muſt find a way to detach all at once the leather ring, which is faſtened to the end of the pole, which the machine I am going to deſcribe will do, after having ſaid that the wheel horſes have each of them faſtened to the breaſt leather of their harneſs a great long leather ring, in which another leather ring is received, in which the end of the pole enters quite to the bottom of the hook.

Fig. 16. the end of the pole is repreſented by AB, and CDE repreſents the hook which is applied to it, and differs from the common ones only in that the end E is flatted, having a ſlit KF in its thickneſs, and pierced quite thro' with a ſquare hole G.

In this ſlit KF between the extremity FK of a piece of iron FKL, which turns upon a hinge in the piece of iron MNO, this piece enters perpendicularly into the pole almoſt to the middle, becauſe the plate of the hook, and the pole are perforated with a hole QR rounded at the two ends, of which the breadth is equal to the diameter of the pin MNO at the bottom, and the length to the quantity with which the pin MNO enters the pole; it is againſt this pin that the ring reſts, which holds the ſplinter-bar to which the fore-horſes are faſtened.

Therefore to detach the fore-horſes from the end of the pole, we need only faſten the piece of iron LKF to the end of the hook E, ſo that it may eaſily undo: this may be done by means of a ſpring ST, of which the end S is held faſt to the plate of the hook by the ſquare rivet V, and the other end T, which is of the breadth of the end of the hook E is perforated by a long ſlit, in which there paſſes a ring H, which holds to the extremity of a ſquare iron pin, which paſſes thro' the 2 thickneſſes of the end of the hook, and of the part FK of the piece FKL, and fills the ſquare hole G.

The iron pin HG is inſtead of the ſlip of leather which holds to the end of the hook the iron pin which paſſes into the end of the pole. Another

other ring I is engaged in the ring H, or a little ſlip is tied in a knot to it, that when the ſpring ST is lowered, it may make the pin HG come out of its hole.

I have ſaid there were two leather rings in which the end of the pole went quite to the bottom of the hook, that is, they occupy the place SP; but as the end of the pole cannot be made to go into theſe rings without lowering the ſpring; ſo they cannot get out without lowering it, and alſo diſengaging at the ſame time the pin HG, which no longer retaining the piece FKLO in the end of the hook, it will be carried away by the leather ring which reſts againſt it, and the fore-horſes will go away with the ſplinter-bar.

We ſee by this conſtruction, that if by the firſt machine we detach the 2 horſes from the pole, they cannot go forward without lowering the ſpring ST, and at the ſame time detaching the ſplinter-bar from the end of the pole, and conſequently the horſes which are faſtened to it.

VII. *A compariſon of the obſervations of the eclipſe of the moon*, Jan. 23, 1712, *in the evening*, *made at* Nuremberg, *by M.* J. P. Wurſelbaur, *and at* Paris, *at the royal obſervatory by Meſſ.* de la Hire*.

We are not to expect ſo great an exactneſs from the compariſon of theſe obſervations, as from thoſe of the eclipſes of the ſatellites of *Jupiter*. And yet we ought not to neglect them, ſince we may obtain from them conſiderable advantages to geography, when we have not an opportunity of obſerving thoſe of the ſatellites; and

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* July. 30, 1712.

Pl. V. *Vol. IV.*

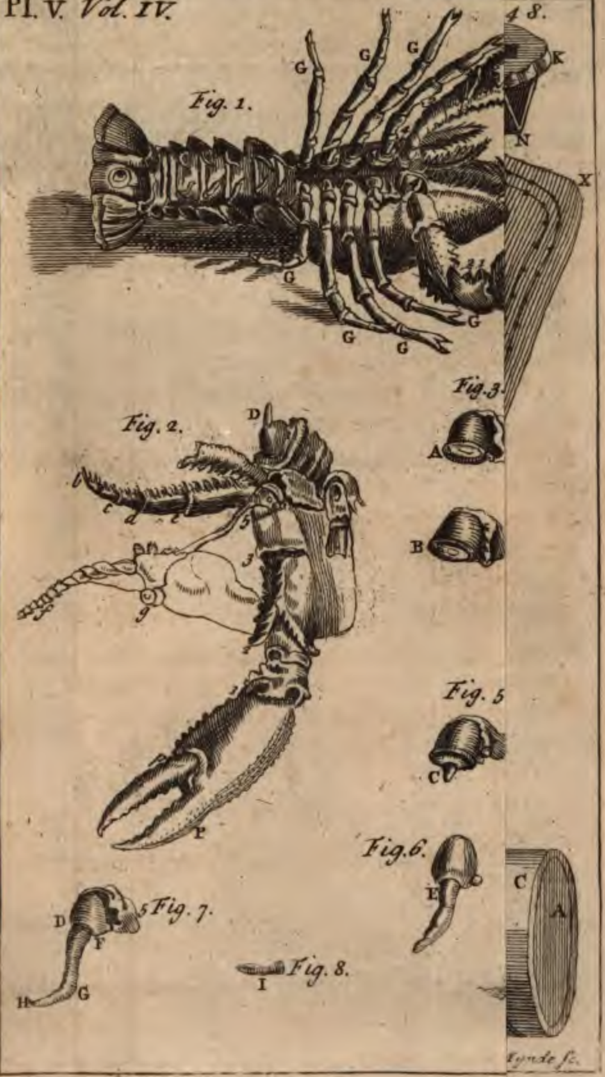
48.
Fig. 1.
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Fig. 3.
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Fig. 2.
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Fig. 5
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Fig. 6.
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Fig. 7.
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Fig. 8.
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eſpecially with regard to places that are very diſtant from each other; and if uſe had been formerly made of thoſe which were obſerved, there would not have been ſuch defective maps made as thoſe which were publiſhed before the academy began to make uſe of the ſatellites of *Jupiter* for the determination of the longitudes.

We have drawn the following table from the obſervations of M. *Wurſelbaur*.

At *Nuremberg.*			Phaſes.		At *Paris.*			Difference.	
h	′	″	°	′	h	′	″	′	″
7	28	40	1	0	6	50	50	37	50
7	32	19	1	30	6	58	50	33	29
7	50	40	3	0	7	25	20	35	20
8	53	35	2	20	8	17	35	36	0
8	56	55	2	0	8	18	19	38	36
9	2	51	1	30	8	30	20	32	31
9	18	10	End		8	44	0	34	10

Paſſage of the ſhadow thro' the ſpots.

h	′	″		h	′	″	′	″
7	31	35	*Heraclides*	6	59	8	32	27
7	38	10	*Plato*	7	4	50	33	20
8	49	10	*Helicon*	8	16	10	33	0
8	53	35	*Harpalus*	8	16	10	37	25
8	58	55	*Plato*	8	24	15	34	40

The magnitude of the eclipſe

At *Nuremberg*	At *Paris*
3 digits, 42′.	3 digits, 40′.

Taking a mean between theſe obſervations, we ſhall have for the difference of the meridians betweeen *Nuremberg* and *Paris* 34′ ½, which is pretty nearly as I determined it in my tables.

A
TABLE
OF THE
PAPERS contained in the ABRIDGMENT of the HISTORY and MEMOIRS of the ROYAL ACADEMY of SCIENCES at PARIS, for the Year MDCCXIII.

In the HISTORY.

I. *Experiments on ſea-coal.*

II. *The hiſtory of the* carcajou, *an* American *animal.*

III. *An obſervation on the areometer.*

In the MEMOIRS.

I. *Meteorological obſervations made at the royal obſervatory, by M.* de la Hire.

II. *Reflections on ſome new obſervations of the flux and reflux of the ſea, made at the port of* Breſt, *in the year* 1712, *by M.* Caſſini.

III. *On the height of the atmoſphere, by M.* de la Hire.

IV. *Of the figure of the earth, by M.* Caſſini.

V. *Experiments and reflections on the prodigious ductility of ſeveral ſubſtances, by M.* de Reaumur.

VI. *Reflections on the obſervations of the tides, by M.* Caſſini.

VII.

VII. *A deſcription of a portable machine, proper to ſupport the glaſſes of very great* foci, *preſented to the academy by M.* Bianchini, *by M.* de Reaumur.

VIII. *A hiſtory of an extraordinary ſleeping, by M.* Imbert.

AN

ABRIDGMENT

OF THE

PHILOSOPHICAL DISCOVERIES and OBSERVATIONS in the HISTORY of the ROYAL ACADEMY of SCIENCES at *Paris*, for the year 1713.

I. *Experiments on ſea-coal.*

M. *Deſlandes* being in *England*, made with the ſea-coal that is burned there, two experiments which he believes have eſcaped the *Engliſh*.

Exp. 1. Having beaten ſome coal, he put about half an ounce of it into a glaſs of water, which became quite black. He left the glaſs expoſed to the air all night upon the window, it was in winter, and the next day he found that the water which was frozen, was of a reddiſh colour. To give this colour to the water, the froſt muſt have diſengaged the ſulphurs of the coal, though this action does not ſeem very well to agree with it.

Exp. 2. Some of the aſhes of this coal infuſed in brandy, and mixed with ſteel filings, makes a black tincture, which clears as it heats. When it begins to boil, it takes a ſofter colour than the common iron-grey. M. *Deſlandes* dyed ſome wool with this agreeable tincture, and no artiſt could imitate it.

II. *The hiſtory of the* carcajou, *an* American *animal.*

M. *Sarraſin*, the king's phyſician at *Canada*, and a correſpondent of the academy, from whom we have ſeen a very curious and exact hiſtory of the *caſtor* or beaver in the memoirs of 1704*, has alſo ſent ſuch another of the *carcajou*, of which we here give an abridgment.

The *carcajou* is a carnivorous animal, which inhabits the coldeſt parts of *North-America*. It commonly weighs from 25 to 35lb. It is about 2 feet from the end of the ſnout to the tail which is about 8 inches long. Its head is very ſhort, and thick, in proportion to the reſt of its body, the eyes very ſmall, the jaws very ſtrong, and furniſhed with 32 ſharp teeth; notwithſtanding it is ſmall, it is very ſtrong and furious, and tho' carnivorous, it is ſo ſlow, and ſo heavy, that it crawls upon the ſnow rather than walks upon it.

As it walks, it can catch no other animal than the beaver which is as ſlow as its ſelf, and that muſt be in ſummer when the beaver is out of its cabbin. But in the winter it can only break and deſtroy the cabbin, and ſurprize the beaver, which but very ſeldom ſucceeds, becauſe the beaver has its ſure retreat under the ice. However, as the beaver, even in winter goes into the woods to ſeek for freſh proviſions, which he likes better than ſtale, the carcajou may attack him there.

The chaſe which is moſt ſucceſsful to him is that of the elk, and *Caribou*, or *Canada* ſtag. The elk chuſes in winter a place where there grows a quantity of *Anagyris fœtida*, or ſtinking bean trefoil, becauſe it feeds upon it, and when the ground is covered 5 or 6 feet with ſnow, he makes roads in theſe places which he never quits, unleſs he is purſued by the hunters. The *carcajou* having obſerved the elk's road, climbs up into a tree near which he muſt paſs, and from thence leaps upon him, and cuts his throat in a moment. In vain does the elk lie upon the ground, or rub himſelf againſt the trees, for nothing will make the carcajou let go his hold, and the hunters have ſometimes found pieces of his ſkin as large as one's hand, which have ſtuck to the tree againſt which the elk had rubbed himſelf.

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* Vol. II. pag. 181. of this abridgment.

The *caribou* is a ſort of ſtag. It is very light, and runs upon the ſnow almoſt as faſt as upon the ground, becauſe his nails which are very broad, and furniſhed with rough hairs in their intervals, hinder him from ſinking, and ſerve him inſtead of the *raquette* † of the ſavages. When it inhabits the thick woods, it makes roads in winter like the elk, and is in the ſame manner attacked there by the *carcajou.* But when it is in open places, where it has not need of making roads, and where it goes indifferently on all ſides, the *carcajou* which might wait too long without ſucceſs, is not accuſtomed to looſe his time, and does not chaſe the *caribou* but in thick places; ſo ingenious is his ardor for his prey.

III. *An obſervation on the areometer.*

As F. *Feuillée* a minim, correſpondent of the academy, read to it the relation of a voyage which he had juſt made into *South-America,* and as he ſpoke of the obſervations which he had made with the areometer with the weight of the ſea-water in different places, it was objected to him, that in a warmer climate the glaſs of the areometer muſt dilate, and conſequently occupies more ſpace in the water that is weighed, and makes it appear leſs heavy than it really is. But M. *Caſſini* anſwered, that the water itſelf was alſo more dilated by the heat, and actually weighed leſs.

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† A ſort of broad ſhoes which the ſavages wear to keep their feet from ſinking into the ſnow.

A N
ABRIDGMENT
OF THE
PHILOSOPHICAL MEMOIRS of the ROYAL ACADEMY of SCIENCES at *Paris*, for the Year 1713.

I. *Meteorological obſervations made at the royal obſervatory, by M.* de la Hire*.

THESE are the obſervations of the quantity of rain-water, and melted-ſnow, with the changes of the weather marked by the thermometer and barometer during all the laſt year 1712. All theſe obſervations have been made as in the preceding years, in the ſame place, and with the ſame inſtrument. The height of water fallen in.

	Lin.		*Lin.*
Jan.	20 $\frac{1}{8}$	July	36 $\frac{1}{2}$
Feb.	8 $\frac{1}{2}$ $\frac{1}{4}$	Aug.	6
March	6 $\frac{1}{4}$	Sept.	39 $\frac{1}{4}$ $\frac{1}{8}$
April	51 $\frac{1}{8}$	Oct.	25 $\frac{1}{2}$ $\frac{1}{4}$
May	12 $\frac{1}{4}$ $\frac{1}{8}$	Novem.	16 $\frac{1}{4}$
June	23 $\frac{1}{8}$	Dec.	8 $\frac{1}{2}$ $\frac{1}{8}$

The ſum of the height of water for the whole year, 254 lines $\frac{1}{4}$, or 21 inches, 2 lines $\frac{1}{4}$, which is more than the mean years, which we have determined at 19 inches.

My thermometer was at the loweſt the laſt day of the year, and marked 24 $\frac{3}{4}$ of its parts pretty near the ſame as the 8th of *Jan.* which ſhews, that the cold has not been great, for it often falls to 14, and in the mean ſtate it is at 48, as in the bottom of the caves of the obſervatory, where it always remains at the ſame point.

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* Jan. 7, 1713.

This thermometer was at the higheſt at 64 parts the 16th of *Auguſt*; but as that was at ſun-riſing, the time when I always make theſe obſervations, and as in the greateſt heat of the day, which is towards 2 in the afternoon, it roſe above the ſtate it was at in the morning 12 parts, it muſt be conſidered as 76 for the greateſt heat, and conſequently the difference marked between the greateſt cold, and the greateſt heat will be 52 parts, the half of which is 26, which being added to 24, makes 50, which is not far from 48; by which we know, that the cold has been very near as much below the mean ſtate, as the heat has been above it.

My common barometer was at the higheſt at 28 inches, 4 lines, $\frac{2}{3}$, the 10th of *Feb.* and about that time it was always very high. The heavens were then pretty ſerene, and very little north winds; and I obſerved alſo, that whenever this barometer was higher than 28 inches, which happened pretty often in the year, the wind was towards the north and eaſt, and ſometimes with fogs. I have another barometer where the quickſilver is always 3 lines higher than in that with which I commonly obſerve. This common barometer was at the loweſt once only, the 6th of *Nov.* at 26 inches, 10 lines, $\frac{2}{3}$, the ſky being ſerene with a moderate eaſt-wind; but the quickſilver preſently roſe again, and the wind turned toward the weſt and ſouth weſt; the difference between the greateſt and leaſt height of this barometer, was 1 inch, 6 lines, as uſual.

There was nothing remarkable in the winds of this year; but I obſerved in general, that in this country, every time the weſt and ſouth-weſt wind prevailed for ſome time, the heavens were cloudy toward the evening, and at the beginning of the night; and that toward the morning it was ſerene. I think the reaſon of it is plain enough; for in the afternoon, the ſun falling almoſt perpendicularly upon the ſeas, which are to the weſt of us, raiſes a great many vapours from them, which are afterwards brought to us about the beginning of the night; on the contrary, there ariſe but few vapours from theſe ſeas in the night time, and the wind continuing the ſame, the ſky muſt be pretty clear toward the morning.

Remarks.

It generally happens that thoſe which have been wounded in any part of the body feel pains there every time the weather is diſpoſed to change. I think it may be accounted for in this manner: the texture of the affected part muſt be very fine, inſomuch that it cannot be touched without feeling of pain; and in the change of the weather, the air becoming either lighter or heavier, makes an extraordinary impreſſion upon theſe parts, either by compreſſing or extending them, as if they were touched by it, which may cauſe the pain which is there felt.

Of the declination of the needle.

We found the declination of the needle to be 11 degrees, 15 minutes, *Dec.* 30. This obſervation was made with the needle of 8 inches long, which we commonly uſe, and in the ſame place as in the preceding years, which is a thick pillar placed at the end of the terraſs of the obſervatory toward the ſouth. The ſide of the box of the compaſs is applied againſt one of the faces of this pillar; and it has been for a long time confirmed, that this face was exactly turned toward the weſt, by putting a thick rule againſt it which bears two ſights at its extremities, to ſee if the rays of the ſun, which paſs through theſe ſights, agree with the true noon marked by the great pendulum clocks regulated by the ſun, which was found very juſt; for this building had been placed with a great deal of care and precaution, by the late M. *Picard*.

We obſerved alſo at the ſame time, the declination of the loadſtone with another needle of only 4 inches long, and found it the ſame with that of 8 inches.

II. *Reflections on ſome obſervations of the flux and reflux of the ſea, made at the port of* Breſt, *in the year* 1712; *by M.* Caſſini*.

Since the royal academy of ſciences has undertaken to examine the phænomena of the flux and reflux of the ſea, we have received a great number of obſervations made in ſeveral parts of *France*, which have ſerved to find new rules, as well to eſtabliſh the time of the tides in each of theſe ports, as to determine their different heights: the greateſt part of theſe obſervations ſeem to prove that there is a great relation between the motions of the moon, and thoſe of the tides, ſince not only the great and ſmall tides follow pretty exactly the ſeveral phaſes of the moon, but even the different heights which are there obſerved are in proportion to the ſeveral diſtances of the moon from the earth.

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* Feb. 1, 1713.

But as we might ſuſpect that theſe various effects had been produced by ſome unknown cauſe, which had concurred at the ſame time with the motions of the moon by a ſort of chance; twas neceſſary to be aſſured of it by a greater number of obſervations.

We have had the opportunity of making them by a new journal of obſervations of the flux and reflux of the ſea made at *Breſt*, which begins the 1ſt of *February* 1712, where the firſt ended, and has been continued to the 12th of *July* of the ſame year.

In this interval of time there have been ſix new and five full moons, the tides of which have been obſerved; that which happened the earlieſt was obſerved the 6th of *February* at $3^{h}\ 9'$ in the morning; and that which happened the lateſt was obſerved *April* the 6th at $4\ 12''\frac{1}{2}$ PM. with an hour, and three minutes difference from one to the other.

However if we ſuppoſe the mean time of the high tide at *Breſt* in the new and full moons to be $3^{h}\ 45'$. the ſame that was determined in the preceding memoir, and make uſe of the common equation of two minutes, for each hour, that the mean time of the high water is faſter or ſlower, with regard to that of the new or full moon, we ſhall find that the high tide ought to have happened *February* 6th, the day of the greateſt acceleration at $3^{h}\ 7'\frac{1}{2}$ in the morning, within about $1\frac{1}{2}$ minute of what was obſerved; and that *April* the 6th, the day of the greateſt retardation, the high tide ought to have happened at $4^{h}\ 15'$ within about $2\frac{1}{2}$ minutes of what was obſerved.

The other obſervations which are to the number of 15, agree for the moſt part to the calculation within a few minutes, and the fartheſt diſtant are only 14 minutes from it, which is an exactneſs to which we could not hope to have arrived if we conſider that it is often difficult to be aſſured within a quarter, or even almoſt half an hour of the high-water.

With regard to the time of low-water, obſerved in the ſame phaſes of the moon, we find that it agrees alſo pretty exactly to the calculation, with this difference only, that the ſea takes up ſome minutes more in falling than in riſing, as we have already obſerved. This difference may riſe at *Breſt* to about a quarter of an hour in the new and full moons, and to half an hour in the quadratures; and this rule is ſo generally obſerved, that of all the obſervations that we have examined for above the ſpace of a year, there are only four or five which are not conformable to it.

As to the time of the high-water obſerved in the quadratures, it is ſubject to more irregularites than in the new and full moons.

The high-water which happened the earlieſt, was obſerved the 29th of *March* at 8h 8′ in the morning, and that which happened the lateſt was obſerved the 28th of *April* at 10h 11′ at night, with a difference from one to the other of 2h 3′. But theſe differences are partly corrected, by ſuppoſing the mean time of the high water at *Breſt* in the quadratures at 8h 57′, the ſame that was determined in the preceding memoir, and making uſe of the common equation of 2 ½ minutes for each hour, that the mean time of the high water is faſter or ſlower with regard to the time of the quadratures. For we ſhall find that the high tide ought to have happened the 29th of *March*, the day of the greateſt acceleration at 8h 34′ in the morning, within about 26 minutes of what was obſerved, and that the 28th of *April*, the day of the greateſt retardation, the high tide ought to have happened at 9h 46′ at night, within about 25 minutes of the obſervation.

We have obſerved in the preceding memoirs, that the retardation of the tides is greater toward the quadratures, than toward the new and full moons. This is conformable to our obſervations, and ſeems ſurpriſing when we conſider, that in the new and full moons, the ſea riſes ſometimes at *Breſt*, to the height of 21 feet, whereas in the quadratures it ſometimes riſes only 4 or five feet, and ſeldom to 11 feet.

However if we ſuppoſe that the motion of the tides is made by a ſort of impulſe, as we have great reaſon to imagine; we find that experience may agree with reaſon; for the preſſure of the air upon the ſea, may be ſuch as not only to make the ſea riſe to a greater degree of height in the new and full moons than in the quadratures, but alſo to make it arrive to this greater height with more velocity.

With regard to the time of the greateſt tides in each lunation, we find that it happens the moſt often at *Breſt* a day after the new or full moon, as the ſmalleſt tides happen alſo a day after the quadratures. We have obſerved at *Dunkirk* and *Havre de Grace*, that the great and ſmall tides there, generally happen two days after theſe phaſes of the moon, ſo that it ſeems that the preſſure which is made upon the ſea in the new and full moons, and in the quadratures is communicated ſooner at *Breſt* than at *Havre de Grace* and *Dunkirk*, which appears conformable to reaſon; the weſtern extremity of *Bretagne*, where *Breſt* is ſituated, being much more advanced toward the ocean where the preſſure is made, than the ports of *Dunkirk* and *Havre*, which are both in the *Engliſh* channel.

We have remarked already that the various heights which we obſerve in the tides, follow pretty exactly the various diſtances of the moon from the earth; that when the moon at the time of new or full, is near its *perigeum*, the tide is greater than in the next *ſyzygia*, where it is near its *apogeum*, this is confirmed by theſe laſt obſervations; for the 4th of *June* 1712, the day of the new moon, this planet was near its *apogeum*, its diſtance from the earth being 1064 parts, the mean of which is 1000. Beſides they obſerved that day, which was the day of the greateſt tide, the height of the full ſea to be 16 feet 2 inches, above a fixed point, which they had taken for the bound of the meaſures, and that of low-water 2 feet 0 inches, which gives the quantity of the elevation of the tide 14 feet 2 inches. The 19th of *June* the day after the full moon, this planet was near its *perigeum*, its diſtance from the earth being 935 parts, the mean of which is 1000. They alſo obſerved the 21ſt of *June*, the day of the greateſt tide, the height of the full ſea to be 18 feet 4 inches, above the fixed point; and the height of the low-water 10 inches below this point, which gives the elevation of the tide 19 feet, 2 inches, greater by 5 feet, than in the preceding obſervation, when the moon was near its *apogeum*.

Tho' all theſe obſervations agree in proving that the various diſtances of the moon from the earth contribute very much to the various elevations that are obſerved in the tides; we do not pretend that they are the ſole cauſe of all the variations that are there obſerved; and it even appears, that there are other cauſes which may concur in making the height of the tides increaſe or diminiſh. We do not here ſpeak of thoſe accidental cauſes, which it would be difficult to give rules to; as for example, of the force and ſituation of the winds, of the different direction of the ſea-coaſt, which not only may accelerate or retard the time of the tides, but may alſo cauſe different elevations to it. But we only undertake to determine thoſe which have ſome regular period.

Now in examining all the obſervations which have been made from the 6th of *Feb.* 1712, to the 12th of *July* of the ſame year, we find that the higheſt tide happened the 24th of *March* at night, and the 25th in the morning when the ſea roſe to the height of 19 feet, 1 inch. The low-water was obſerved the 24th in the morning, 1 foot, 6 inches, below the fixed point, ſo that the elevation of the ſea was, the 24th of *March*, 20 feet, 7 inches. The moon was then nearer its *perigeum* than its *apogeum*, its diſtance from the earth being 977 parts, the mean of which is 1000; but we cannot attribute the whole of this elevation of the tide to the proximity of the moon with regard to the earth, ſince the height of the tide was obſerved the 24th of *March*, greater by 1 foot, 5 inches, than *June* 21, the time when the moon was much neareſt its *perigeum*; it appears therefore that there was in the month of *March*, ſome other cauſe which contributed to the elevation of the tide; and as this obſervation was made near the vernal equinox, which happened the 20th, at 11^h 19′ P.M. and that of *June* 21, near the ſummer ſolſtice which happened at 11^h 17′ in the morning: this has given us room to conjecture that all things being equal, the tides are greater in the equinoxes than in the ſolſtices.

In the next new moon which happened on the 6th of *April*, the moon was nearer its *apogeum* than its *perigeum*; its diſtance from the earth being 1032, of which the mean is 1000; and *April* the 7th in the morning, they obſerved the full ſea to be 18 feet, 2 inches; and that of the low-water to be 5 inches, which gives the elevation of the ſea for that day 17 feet, 7 inches, ſmaller by three feet than the 24th of *March*. And it ought to be ſo for two reaſons; one of which is, that the moon was farther diſtant from the earth the 7th of *April*, than the 24th of *March*, and the other that it was farther diſtant from the equinoctial.

Theſe obſervations are confirmed by thoſe which were made at *Breſt* the preceding year. For the 30th of *June*, 1711, the diſtance of the moon from the earth being 960, the elevation of the tide was obſerved the 1ſt of *July* to be 18 feet, 1 inch, leſs by 2 feet, 10 inches, than the 14th of *Sept.* near the equinox, when it was obſerved to be 20 feet, 11 inches, the diſtance of the moon from the earth being the 12th of *Sept.* the day of the new moon 969, that is, very little different from that of the 30th of *June*.

It appears therefore by theſe obſervations, that the different heights that are obſerved in the tides, depend upon two cauſes, whereof the principal, which hitherto is moſt confirmed by our obſervations, is the various diſtance of the moon from the earth; the ſecond is its proximity or diſtance from the equinoctial; and that the combination of theſe two cauſes produces the chief *phænomena* which we obſerve in the height of the tides.

It follows from hence, 1ſt, That when the new or full moon meets in its *perigeum*, and at the ſame time in the equinoxes, then the tide which immediately follows, is the higheſt that is poſſible.

2d. That when the new and full moon meet in the equinoxes, towards the mean diſtances, then the height of the tides is greater than in the new or full moons, which happen toward the mean diſtances, and near the *ſolſtices*.

3d. That when the new or full moon meets in its*apogeum*, and at the ſame time in the *ſolſtices*, then the high-water is the loweſt that is poſſible. The two firſt rules agree with that which we have hereabove marked, and the laſt is confirmed by the obſervation of the 4th of *June* 1712, for the moon being then near its *apogeum*, and the ſummer *ſolſtice*, the height of the full ſea was obſerved to be 16 feet, 2 inches, and that of the low-water 2 feet, 0 inches, which gives the elevation of the tide 14 feet 2 inches, which is the leaſt that has been obſerved at the new and full moons in above a year. The 5th of *July* following the ſun was at equal diſtance from the *ſolſtice*, but the diſtance of the moon from the earth was 1061 a little leſs than the 4th of *June*, which ought to have cauſed a greater elevation in the tide, as in effect was obſerved. For the 5th of *July* in the morning, the height of the full ſea was obſerved 16 feet 3 inches, and that of the low-water, 1 foot 8 inches, which gives the elevation of the tide 14 feet 7 inches, greater by 5 inches than the 4th of *June*.

As to the ſmall tides which follow the quadratures, they obſerve the ſame that we have done in the preceding memoirs, that their various elevations depend partly upon the various diſtance of the moon from the earth. For example, the 14th of *February* 1712, the day of the firſt quarter, the moon being near its *apogeum*, and its diſtance from the earth 1062, the height of the full ſea was obſerved the 15th of *February* at night, 10 feet 9 inches 6′, and the height of the low-water 5 feet 2 inches, ſo that the elevation of the tide that day was only 5 feet 7 inches 6′. The twenty-ninth of *February* following, the day of the third quarter, the moon being near its *perigeum*, and its diſtance from the earth 975, they obſerved, the 2d of *March* in the morning, the height of the full ſea, 11 feet 9 inches, greater by 11 inches 6 lines than the 15th of *February*.

The 15th of *March*, the day of the firſt quarter, the moon being near its *apogeum*, and its diſtance from the earth 1063, the height of the full ſea was obſerved the 16th of *March* in the morning 10 feet 10 inches, and the height of the low-water 6 feet 4 inches; ſo that the elevation of the ſea that day, was only 4 feet 6 inches, a little leſs than the 15th of *February*, which ought to happen, the moon being then a little nearer the earth, than in the preceding obſervation of the month.

In the quadratures which happened when the moon was almoſt at equal diſtance from the earth, they obſerved almoſt the ſame height in the elevation of the tides. For the 12th of *June*, the day of the firſt quarter, the diſtance of the moon from the earth being 1020, they obſerved that 12 in the morning, which was the day of the ſmalleſt tide, the height of the full ſea 12 feet 9 inches, and that of the low-water 3 feet 5 inches, ſo that the elevation of the ſea, was that day 9 feet, 4 inches.

The 25th of *June* following, the day of the laſt quarter, the moon's diſtance from the earth being 1028, they obſerved the 28th in the morning, the height of the full ſea, 12 feet 5 inches 8′, and that of the low-water, 3 feet 11 inches 4′; ſo that the elevation of the ſea was that day 8 feet 6 inches 4′, a little leſs than that of the 28th of *June*, as it ought to have been obſerved, the diſtance of the moon from the earth, being leſs the 13th of *June*, than the 28th. Theſe two obſervations having been made near the ſummer *ſolſtice*, we have compared them, with thoſe which were made near the equinoxes, and found that all things being equal, the ſmall tides which follow the quadratures, are greater near the *ſolſtices*, than near the equinoxes; for the height of the full ſea, was obſerved the 16th of *March*, the day of the ſmalleſt tide 10 feet 10 inches, the moon being near its *apogeum*, and the 31ſt of *March*, 13 feet 2 inches, the moon being nearer its *perigeum*. Taking a mean, we ſhall have the middle height of the tides, in the equinoxes at *Breſt*, 12 feet, ſmaller by 7 or 8 inches, than the mean height drawn from the obſervations of the 15th and 28th of *June*, made in the *ſolſtices*.

Altho' this effect ſeems contrary to what we obſerved in the new and full moons, the tides of which are greater towards the equinoxes than towards the ſolſtices; we ſee however that it may depend upon the ſame cauſe; for the moon being in one of its quarters at the time of the equinox, runs over by its daily motion a parallel circle, a little diſtant from the tropicks, and the preſſure that it cauſes upon the ſea, being made by a ſmall circle, muſt be leſs felt. On the contrary the moon being in one of its quarters at the time of the ſolſtice, runs over by its daily motion, the equinoctial, or a parallel very little diſtant from it, and conſequently, its preſſure upon the waters of the ſea, which is made according to a great circle of the earth, muſt be greater than when it run over one of the tropicks.

It appears by this that the ſeveral heights which are obſerved in the tides of the equinoxes and ſolſtices, muſt not be regulated exactly by the time of the equinoxes and ſolſtices, but by the greater or leſs declination of the moon, with regard to the equinoctial; for the ſun being in the vernal equinox, and the moon in its laſt quarter, that is in the ſouthern ſigns, the height of the tide muſt be much leſs, when the latitude of the moon is ſouthern than when it is northern; and for the ſame reaſon, the ſun being in the ſummer ſolſtice at the time of the new moon, the height of the tide muſt be much ſmaller when the latitude of the moon is northern, than when it is ſouthern.

From theſe obſervations we may draw theſe two general rules.

1ſt, That all things being equal, the tides ought to be leſs, when the moon, being in its *apogeum* and in its ſouthern ſigns, its latitude is at the ſame time ſouthern; or elſe when the moon being in its *apogeum*, and in the northern ſigns, its latitude is alſo northern.

2d, That on the contrary, the tides muſt be greater, when the moon being in its *perigeum* runs over the equinoctial, without any declination.

It is difficult to find obſervations that have been made exactly, in theſe different circumſtances; it will be ſufficient for us to obſerve that the 16th of *June* 1711, the day of the new moon, the diſtance of the moon from the earth being 1048, that is the moon being near its *apogeum*, with a ſouthern declination of 26° 36′, which is the greateſt that has been found in more than a year, the height of the full ſea was obſerved the 17th, to be 16 feet 2 inches 6′, which is one of the ſmalleſt that has been obſerved. The 4th of *June*, the year following, the day of the new moon, the diſtance of the moon from the earth being 1064, and the northern declination of the moon 25° 10, the height of the greateſt tide was obſerved that day, 16 feet 2 inches, which is the leaſt that we have found at *Breſt*, in the obſervations of the new and full moons.

The ſame effect is obſerved in the quadratures, for the 5th of *September* 1711, the day of the laſt quarter, the moon's diſtance from the earth being 1061, and the northern declination of the moon 26° 31′, which is one of the greateſt that has happened from the 23d of *June* 1711, to the 11th of *July* 1712. The height of the ſmalleſt tide was obſerved the 6th of *September*, 10 feet, 3 inches, which is the loweſt that has been obſerved for above a year; the height of the low-water was obſerved at night, to be 5 feet 11 inches, ſo that the elevation of the ſea that day was only 4 feet 4 inches.

The 15th of *March*, the following year, 1712, the day of the laſt quarter, the diſtance of the moon from the earth being 1063, pretty near the ſame as the 5th of *September*, and the moon's northern declination 25° 45′, a little leſs than in the obſervation of the preceding year, the height of the ſmalleſt tide was obſerved the 16th of *March*, 10 feet 10 inches, a little greater than the 5th of *September* 1711, but one of the loweſt that had been obſerved, the elevation of the ſea that day being only 4 feet 6 inches.

As to the great tides which ought to happen when the moon is in its *perigeum*, and runs over the equinoctial, we have no obſervations at *Breſt* that have been made in theſe circumſtances; it will be ſufficient to remark, that the 12th of *September* 1711, the diſtance of the moon from the earth being 969, and its declination with regard to the equinoctial 2° 39′, which is the ſmalleſt that happened in the new and full moons, from the 16th of *June* 1711, to the 11th of *July* 1712, the height of the full ſea was obſerved the 14th of *September*, 18 feet 11 inches, and that of low-water 2 feet below the fixed point, which gives the elevation of the tide for that day 20 feet 11 inches, which is one of the greateſt that happened at *Breſt*, in that interval of time.

We may here obſerve that the difference between the circles of declination from one degree to another, continually increaſes by receding from the equator, and approaching to the pole; and that conſequently the variation of the declination of the moon, muſt produce a leſs ſenſible effect upon the tides near the equinoctial than towards the tropicks.

The obſervations here above related, ſeem ſufficiently to prove that the different declinations of the moon, with regard to the equinoctial, contribute to increaſe or diminiſh the heights of the tides, as well as the various diſtances of the moon from the earth. Theſe two cauſes being generally complicated, it is neceſſary in order to diſtinguiſh them, to have a greater number of obſervations, made in different ſituations of the moon, as well with regard to the earth, as with regard to the equinoctial; for this reaſon we have thought it neceſſary to examine ſome which were made at *Breſt* in 1692.

The journal of theſe obſervations begins *June* 6, 1692, and ends the laſt of *October* of the ſame year. They have there marked every day the height of the full ſea, with regard to a rock called the *Roſe*, which is at the entrance and within the port, and have alſo added the temperature of the air, and the ſituation and force of the wind.

By the examination that we have made of all theſe obſervations, we have found, that in this interval of time the greateſt tide happened the

12th of *September* 1692, two days after the new moon, the ſea being that day riſen to the height of 28 feet 7 inches. Having calculated the ſituation of the moon for the time of the preceding new moon, which happened the 10th of *September*, at 6h 12′ PM. we found that this planet was very near its *perigeum*, its diſtance from the earth being 936 parts, the mean of which is 1000; the moon was alſo very near the equinoctial, its ſouthern declination being only 3° 11′, which could cauſe very little variation in the height of the ſea.

In the other obſervations of the tides, made at the new or full moons; this planet was not only farther diſtant from the earth, than on the 10th of *September*, but even its declination, with regard to the equinoctial, was greater; thus all the tides ought to be ſmaller than the 12th of *September*, which is conformable to the obſervations.

In the full moon following, which happened the 25th of *September*, at 10h 54′ in the morning, the moon was in its *apogeum*, its diſtance from the earth was 1065, and its northern declination 4° 59′. The 25th they alſo obſerved the height of the ſea, to be 25 feet 5 inches, 3 feet 2 inches lower than the 12th of the ſame month.

The 14 of *June* the ſame year, the day of the new moon, the diſtance of the moon from the earth being 993, the mean of which is 1000, and its northern declination being 27° 10′, which is the higheſt that happened, they obſerved the 16th the height of the ſea, 25 feet 7 inches, lower by 1 foot 5 inches, than the mean height of the tides, drawn from obſervations made in the month of *September*, when the moon was near the equator.

As to the ſmall tides obſerved at *Breſt*, in the quadratures, we find that the greateſt happened the 23d of *June*, two days after the firſt quarter; the ſea being riſen that day to the height of 21 feet 4 inches. The moon was then near its *perigeum*, its diſtance from the earth being 977, it was alſo very near the equinoctial, its northern declination being only 3° 42′.

The 8th of the preceding *June*, the height of the ſea was obſerved 19 feet 1 inch, leſs by 2 feet 3 inches than the 23d of *June*, as they ought to have obſerved, the moon being the 6th of *June*, the day of the laſt quarter, in its *apogeum*, and its diſtance from the earth 1064.

We ſee therefore by theſe obſervations made at *Breſt* in 1692, as well as by thoſe that have been made the laſt years in the ſame port, that the diſtance of the moon from the earth, and its declination, with regard to the equator, contribute very much to the increaſe and diminution that are obſerved in the heights of the tides.

Upon theſe principles we have prepared tables, to find at *Breſt*, the height of the tides, as well in the new and full moons, as in the quadratures.

For this, we ſuppoſe that at *Breſt*, in the great tides which follow the new or full moons, when the planet is in its *perigeum*, and at the ſame time runs over the equinoctial, the height of the full ſea is 20 feet; and that when the moon is in its *apogeum*, and upon the equinoctial, the height of the full ſea is 17 feet 0 inches; and that when it is in its *apogeum*, and its declination with regard to the equinoctial is 28° 50′, which is the greateſt that it can have, the height of the full ſea is 15 feet, 6 inches. We alſo ſuppoſe that in the ſmall tides which follow the quadratures, when the moon is in its *perigeum*, and runs over the equinoctial, the height of the full ſea is 14° 0′; that when this planet is in its *apogeum*, and upon the equinoctial, the height of the full ſea is 12 feet; and that when it is in its *apogeum*, and its declination is 28° 50′, the height of the full ſea is 10° 6′.

According to theſe rules, the difference of the height of the full ſea cauſed by various diſtances of the moon from the earth in the new and full moons, is three feet, twice as great as that which is produced by the declination of the moon with regard to the equinoctial.

But as the difference between the diſtance of the moon from the earth in its *apogeum*, and in its *perigeum*, is ſmaller by $\frac{1}{3}$ in the quadratures than in the new and full moons, they have ſuppoſed that the difference of the height of the full ſea cauſed by various diſtances of the moon from the earth in the quadratures, is only two feet, conformable to the obſervations.

In the other ſituations of the moon with regard to the earth; the height of the full ſea is proportionable to the various diſtances of the moon from the earth. To find the variations cauſed by the declination of the moon with regard to the equinoctial, we have taken declinations of which the ſines of the complements are in arithmetical proportion, that we may be able to diſtribute equally the height of the tides.

We ſhall find by the means of theſe tables, and of the following rules, the height of the great and ſmall tides at *Breſt*, as well in the new and full moons as in the quadratures.

Rule

Rule I.

To find at *Breſt* the height of the greateſt tide, which ought to happen in a given new or full moon.

Seek by the aſtronomical tables the diſtance of the moon from the earth, and its declination with regard to the equinoctial; take at the top of the firſt table the diſtance of the moon from the earth, and at the ſide its declination, and you will find over-againſt it the height of the greateſt tide.

Rule II.

To find at *Breſt* the height of the ſmalleſt tide, which ought to follow one of the quadratures.

Seek by the aſtronomical tables, the diſtance of the moon from the earth, and its declination; take at the top of the ſecond table the diſtance of the moon from the earth, and at its ſide its declination, the height of the tide which anſwers to theſe two figures, will be the height of the ſmalleſt tide which follows the quadrature given.

Examp. 1. We ſeek the height of the greateſt tide which happened in the new moon *October* 1711.

We ſhall find, that the 12th of *October* at 6° 0′ in the morning, the time of the new moon, the diſtance of this planet from the earth was 947; and its ſouthern declination 12° 13′. Take in the firſt table over-againſt 95, and 12° 0′, the height of the greateſt tide which we ſhall find to be 19° 6′ exactly the ſame a was obſerved.

Examp. 2. We ſeek the height of the greateſt tide which happened in the new moon of *June* 1712.

We find, that the 4th of *June* at 7^h 25′ in the morning, the time of the new moon, the northern declination of that planet was 25° 10′, and its diſtance from the earth 1064. Take in the firſt table over-againſt 106, and 25° 33′, the height of the greateſt tide that we ſhall find 15 feet, 10 inches, only 4 inches leſs than what was obſerved.

Examp. 3. We ſeek the height of the ſmalleſt tide which follows the firſt quarter of the moon the 23d of *June*, 1712.

We find, that the 25th of *June* at 8^h 18′ P.M. the northern declination of the moon was 6° 30′, and its diſtance from the earth 987. Take in the ſecond table over-againſt 99, and 6° 56, the height of the ſmalleſt tide which we ſhall find to be 13 feet, 8 inches, within about 4 lines of what was obſerved.

Examp. 4. We ſeek the height of the ſmalleſt tide which followed the firſt quarter of the moon, *Sept.* 5, 1712.

We ſhall find at that time the moon's diſtance from the earth was 1061, and its northern declination 26° 28′. Take in the ſecond table over-againſt 106, and 26° 22′, the height of the ſmalleſt tide which we ſhall find 10° 9′ 0″, only 6 inches greater than what they obſerved.

We might draw up like rules to find the height of the tides in the other ports of the ocean, provided we had ſeveral obſervations of the full ſea made in different ſituations of the moon, and principally when it is near its *apogeum* and its *perigeum*, when it runs over the equinoctial; or when it is towards its greateſt declination.

A TABLE of the height of the great tides in the new and full moons.

Declination of the moon.	Diſtance of the moon from the earth.												
	94	95	96	97	98	99	100	101	102	103	104	105	106
	Height of the full ſea at Breſt in the new and full moons.												
0 0	20 0	19 9	19 6	19 3	19 0	18 9	18 6	18 3	18 0	17 9	17 6	17 3	17 0
6 56	19 11	19 8	19 5	19 2	18 11	18 8	18 5	18 2	17 11	17 8	17 5	17 2	16 11
9 48	19 10	19 7	19 4	19 1	18 10	18 7	18 4	18 1	17 10	17 7	17 4	17 1	16 10
12 0	19 9	19 6	19 3	19 0	18 9	18 6	18 3	18 0	17 9	17 6	17 3	17 0	16 9
13 50	19 8	19 5	19 2	18 11	18 8	18 5	18 2	17 11	17 8	17 5	17 2	16 11	16 8
15 26	19 7	19 4	19 1	18 10	18 7	18 4	18 1	17 10	17 7	17 4	17 1	16 10	16 7
16 54	19 6	19 3	19 0	18 9	18 6	18 3	18 0	17 9	17 6	17 3	17 0	16 9	16 6
18 14	19 5	19 2	18 11	18 8	18 5	18 2	17 11	17 8	17 5	17 2	16 11	16 8	16 5
19 28	19 4	19 1	18 10	18 7	18 4	18 1	17 10	17 7	17 4	17 1	16 10	16 7	16 4
20 37	19 3	19 0	18 9	18 6	18 3	18 0	17 9	17 6	17 3	17 0	16 9	16 6	16 3
21 42	19 2	18 11	18 8	18 5	18 2	17 11	17 8	17 5	17 2	16 11	16 8	16 5	16 2
22 44	19 1	18 10	18 7	18 4	18 1	17 10	17 7	17 4	17 1	16 10	16 7	16 4	16 1
23 43	19 0	18 9	18 6	18 3	18 0	17 9	17 6	17 3	17 0	16 9	16 6	16 3	16 0
24 39	18 11	18 8	18 5	18 2	17 11	17 8	17 5	17 2	16 11	16 8	16 5	16 2	15 11
25 33	18 10	18 7	18 4	18 1	17 10	17 7	17 4	17 1	16 10	16 7	16 4	16 1	15 10
26 25	18 9	18 6	18 3	18 0	17 9	17 6	17 3	17 0	16 9	16 6	16 3	16 0	15 9
27 15	18 8	18 5	18 2	17 11	17 8	17 5	17 2	16 11	16 8	16 5	16 2	15 11	15 8
28 3	18 7	18 4	18 1	17 10	17 7	17 4	17 1	16 10	16 7	16 4	16 1	15 10	15 7
28 30	18 6	18 3	18 0	17 9	17 6	17 3	17 0	16 9	16 6	16 3	16 0	15 9	15 6

TABLE of the height of the ſmall tides in the quadratures.

Declination of the moon.		*Diſtance of the moon from the earth.*																	
		98		99		100		101		102		103		104		105		106	
		Height of the full ſea at Breſt *in the quadratures.*																	
0	0	14	0	13	9	13	6	13	3	13	0	12	9	12	6	12	3	12	9
6	56	13	11	13	8	13	5	13	2	12	11	12	8	12	5	12	2	11	11
9	48	13	10	13	7	13	4	13	1	12	10	12	7	12	4	12	1	11	10
12	0	13	9	13	6	13	3	13	0	12	9	12	6	12	3	12	0	11	9
13	50	13	8	13	5	13	2	12	11	12	8	12	5	12	2	11	11	11	8
15	26	13	7	13	4	13	1	12	10	12	7	12	4	12	1	11	10	11	7
16	54	13	6	13	3	13	0	12	9	12	6	12	3	12	0	11	9	11	6
18	14	13	5	13	2	12	11	12	8	12	5	12	2	11	11	11	8	11	5
19	28	13	4	13	1	12	10	12	7	12	4	12	1	11	10	11	7	11	4
20	37	13	3	13	0	12	9	12	6	12	3	12	0	11	9	11	6	11	3
21	42	13	2	12	11	12	8	12	5	12	2	11	11	11	8	11	5	11	2
22	44	13	1	12	10	12	7	12	4	12	1	11	10	11	7	11	4	11	1
23	43	13	0	12	9	12	6	12	3	12	0	11	9	11	6	11	3	11	0
24	39	12	11	12	8	12	5	12	2	11	11	11	8	11	5	11	2	10	11
25	33	12	10	12	7	12	4	12	1	11	10	11	7	11	4	11	1	10	10
26	25	12	9	12	6	12	3	12	0	11	9	11	6	11	3	11	0	10	9
27	15	12	8	12	5	12	2	11	11	11	8	11	5	11	2	10	11	10	8
28	3	12	7	12	4	12	1	11	10	11	7	11	4	11	1	10	10	10	7
28	50	12	6	12	3	12	0	11	9	11	6	11	3	11	0	10	9	10	6

III. *On the height of the atmoſphere, by M.* de la Hire*.

Let the circle TNI†, be one of the great circles of the earth, of which the centre is C, and let the meeting of the plane of this circle, with the ſurface of the atmoſphere, which I ſuppoſe ſpherical, be the circle ZPA, of which the centre is alſo in C; let the ſenſible horizon of a point T, of the ſurface of the earth be TL, which ſhall be perpendicular to the vertical line ZTC of the ſame point T.

We know by the obſervations that a ray of light which comes from a ſtar, and makes with the horizon TL, an angle of 32′, underneath, as PM, after having traverſed the atmoſphere, comes to the eye at T, where it appears in the horizon TL; and this ray muſt deſcribe in the atmoſphere a curve, which ſhall be touched at the point T, by the horizon LT; for the ray MP, meeting the ſurface of the atmoſphere obliquely in P, draws nearer the perpendicular CP, by advancing toward T, and turning its concavity toward the earth, becauſe the atmoſphere is a body which continually increaſes in denſity as it approaches the earth. This angle of 32′ is what we call the horizontal refraction.

It follows from hence, that all the luminous points which ſhould be upon the curve PT, within the atmoſphere would appear in the horizon TL, and thoſe which ſhould be underneath would not appear from the point T, and if we would conſider the atmoſphere, as a matter quite homogeneous like water, and as many aſtronomers have done, it is evident the ſame thing would always follow; for then a ray *mp* which ſhould make with TL an angle *mp*L of 32′ by meeting the atmoſphere in *p* would turn aſide by the right horizontal line pT; but this ſuppoſition not being natural, we ſhall here make uſe of the curve PT.

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* Feb. 25, 1713.

† Plate VI. Fig. 1. Page

If thro' the point P we therefore bring the ray of the earth CNPK which meets its ſurface in N, and the horizon TL in K, and take an arch NI equal to the arch NT, it is evident that IK would be perpendicular to the ſemi-diameter of the earth CI; and that the eye being placed at I would ſee all the luminous points which ſhould be within the atmoſphere, and in a curve IP like TP, in the line IK which would be horizontal from the point I, as TK is from the point T.

But moreover if we alſo imagine beyond I another curve IA within the atmoſphere, and which is equal and ſimilar to IP or TP, it follows that a ray of light as DA, which ſhould meet the atmoſphere in A, and make with KI prolonged an angle of 32′, would traverſe the atmoſphere by the curve AI in touching the circumference of the circle of the earth TNI in I, and would purſue its way by the other curve IP, and would go out of the atmoſphere at P, making with IK an angle of 32′.

Now let us ſee the effect of the rays of the ſun in the atmoſphere.

All aſtronomers agree that when the centre of the ſun is 18 degrees below the horizon, we ſee the beginning or the end of the twilight; and thoſe who have obſerved the twilight in ſerene cold weather, take notice that its light is diſtinct enough toward the horizon to make an exact de-

A TABLE of the height of the great tides in the new and full moons.

Declination of the moon.	*Distance of the moon from the earth.*												
	94	95	96	97	98	99	100	101	102	103	104	105	106
	Height of the full ſea at Breſt *in the new and full moons.*												
0 0	20 0	19 9	19 6	19 3	19 0	18 9	18 6	18 3	18 0	17 9	17 6	17 3	17 0
6 56	19 11	19 8	19 5	19 2	18 11	18 8	18 5	18 2	17 11	17 8	17 5	17 2	16 11
9 48	19 10	19 7	19 4	19 1	18 10	18 7	18 4	18 1	17 10	17 7	17 4	17 1	16 10
12 0	19 9	19 6	19 3	19 0	18 9	18 6	18 3	18 0	17 9	17 6	17 3	17 0	16 9
13 50	19 8	19 5	19 2	18 11	18 8	18 5	18 2	17 11	17 8	17 5	17 2	16 11	16 8
15 26	19 7	19 4	19 1	18 10	18 7	18 4	18 1	17 10	17 7	17 4	17 1	16 10	16 7
16 54	19 6	19 3	19 0	18 9	18 6	18 3	18 0	17 9	17 6	17 3	17 0	16 9	16 6
18 14	19 5	19 2	18 11	18 8	18 5	18 2	17 11	17 8	17 5	17 2	16 11	16 8	16 5
19 28	19 4	19 1	18 10	18 7	18 4	18 1	17 10	17 7	17 4	17 1	16 10	16 7	16 4
20 37	19 3	19 0	18 9	18 6	18 3	18 0	17 9	17 6	17 3	17 0	16 9	16 6	16 3
21 42	19 2	18 11	18 8	18 5	18 2	17 11	17 8	17 5	17 2	16 11	16 8	16 5	16 2
22 44	19 1	18 10	18 7	18 4	18 1	17 10	17 7	17 4	17 1	16 10	16 7	16 4	16 1
23 43	19 0	18 9	18 6	18 3	18 0	17 9	17 6	17 3	17 0	16 9	16 6	16 3	16 0
24 39	18 11	18 8	18 5	18 2	17 11	17 8	17 5	17 2	16 11	16 8	16 5	16 2	15 11
25 33	18 10	18 7	18 4	18 1	17 10	17 7	17 4	17 1	16 10	16 7	16 4	16 1	15 10
26 25	18 9	18 6	18 3	18 0	17 9	17 6	17 3	17 0	16 9	16 6	16 3	16 0	15 9
27 15	18 8	18 5	18 2	17 11	17 8	17 5	17 2	16 11	16 8	16 5	16 2	15 11	15 8
28 3	18 7	18 4	18 1	17 10	17 7	17 4	17 1	16 10	16 7	16 4	16 1	15 10	15 7
28 30	18 6	18 3	18 0	17 9	17 6	17 3	17 0	16 9	16 6	16 3	16 0	15 9	15 6

TABLE of the height of the ſmall tides in the quadratures.

Declination of the moon.		Diſtance of the moon from the earth.																	
		98		99		100		101		102		103		104		105		106	
		Height of the full ſea at Breſt in the quadratures.																	
0	0	14	0	13	9	13	6	13	3	13	0	12	9	12	6	12	3	12	9
6	56	13	11	13	8	13	5	13	2	12	11	12	8	12	5	12	2	11	11
9	48	13	10	13	7	13	4	13	1	12	10	12	7	12	4	12	1	11	10
12	0	13	9	13	6	13	3	13	0	12	9	12	6	12	3	12	0	11	9
13	50	13	8	13	5	13	2	12	11	12	8	12	5	12	2	11	11	11	8
15	26	13	7	13	4	13	1	12	10	12	7	12	4	12	1	11	10	11	7
16	54	13	6	13	3	13	0	12	9	12	6	12	3	12	0	11	9	11	6
18	14	13	5	13	2	12	11	12	8	12	5	12	2	11	11	11	8	11	5
19	28	13	4	13	1	12	10	12	7	12	4	12	1	11	10	11	7	11	4
20	37	13	3	13	0	12	9	12	6	12	3	12	0	11	9	11	6	11	3
21	42	13	2	12	11	12	8	12	5	12	2	11	11	11	8	11	5	11	2
22	44	13	1	12	10	12	7	12	4	12	1	11	10	11	7	11	4	11	1
23	43	13	0	12	9	12	6	12	3	12	0	11	9	11	6	11	3	11	0
24	39	12	11	12	8	12	5	12	2	11	11	11	8	11	5	11	2	10	11
25	33	12	10	12	7	12	4	12	1	11	10	11	7	11	4	11	1	10	10
26	25	12	9	12	6	12	3	12	0	11	9	11	6	11	3	11	0	10	9
27	15	12	8	12	5	12	2	11	11	11	8	11	5	11	2	10	11	10	8
28	3	12	7	12	4	12	1	11	10	11	7	11	4	11	1	10	10	10	7
28	50	12	6	12	3	12	0	11	9	11	6	11	3	11	0	10	9	10	6

becauſe this curvity muſt have its concavity turned toward the earth, and this refracted ray *q* T being bent, would divaricate from the direct ray *ſq* by receding from the perpendicular C*q* drawn to the ſurface of the atmoſphere, whereas it ought to approach it; it will paſs therefore above *q* toward K.

Let us now ſee by the means of the given angles, and by the magnitude of the radius or ſemi-diameter of the earth, which is known, what are the magnitudes of the lines NK and N*q*.

We have in the triangle CTK rectangular in T, the angle TCK of 8° 36′, therefore its complement CKT will be 81° 24′; and we know the ſemi-diameter of the earth CT 3,269,297 toiſes, we ſhall find therefore CK of 3,306,520 toiſes, from which taking away CN or CT, there will remain NK of 37,223 toiſes.

Again, in the triangle CT*q* we have the angle TC*q* known as above to have 8° 36′; but the angle C*q*T is leſs than a right one by 36′, ſince KT*q* is ſuppoſed to be 36′, therefore CT*q* will be 89° 24′; and laſtly, the angle CT*q* will be 82° 0′; and CT is the ſame as in the other; we ſhall find therefore C*q* 3,301,244 toiſes, from which taking away the value of CN, there will remain for N*q* 31,947 toiſes.

It therefore follows from hence, that the height of the atmoſphere is leſs than 37,223 toiſes, and greater than N*q* which is 31,947 toiſes; and if we take the mean which cannot be far from the truth, we ſhall have the height of the atmoſphere NP, 34,585 toiſes.

M. *Mariotte* in his eſſay on the nature of the air, concludes the height of the atmoſphere a little leſs than this by his principle of the condenſation of the air in its different heights, and upon experiments:

riments: but theſe ſorts of calculations can never be very juſt, becauſe they are deduced from ſome gravity of the air near the earth; and beſides we cannot know by our experiments exactly to what height the elaſtic particles of the air can dilate themſelves in the *æther*, nor the progreſſion of their dilatation; and it is a good deal to have come ſo near as he has done.

Kepler in his *Epitomy of Aſtronomy* determines the height of the atmoſphere by the *crepuſcula* according to the idea of the ancients, who conſidered only direct rays which met the atmoſphere, after having touched the earth, without having regard to the refraction; and he finds it by a calculation which he makes of 10 *German* miles, each of which is equal to about 3,800 of our toiſes; and the height of the atmoſphere would therefore be 38,000 toiſes, which is more than we have found it, and much more than he believed it; for he reckoned its true height only at a little more than half a *German* mile, which would be almoſt 2,000 toiſes. There is a great probability that ſo conſiderable a difference made him ſeek the means of explaining the twilight, by employing thereupon ſome reflections within the atmoſphere which he adds to the refraction; and by a matter which he imagines to be about the ſun, and to be illuminated by it; and he inſiſts ſtrongly upon this thought, for he relates many reaſons to ſupport it, one of the principal of which is, the curved figure of the twilight which is obſerved in the cold and ſerene nights; he alſo adds, that this apparent figure is a ſegment of a circle which terminates at the horizon; but I am going to demonſtrate that it is not a portion of a circle, but an hyperbola which is a little altered by the refraction, and that it is not neceſſary for the explaining this curved figure of the *crepuſculum*, to have recourſe to a matter which encompaſſes the ſun, and that it is only the atmoſphere which muſt produce it, for ſuch a matter would very much interrupt the celeſtial *phænomena*.

† Let there be as before, the vertical line CT, which paſſing thro' a point T of the ſurface of the earth, meets the ſurface of the atmoſphere ZX at the point Z, and let TL be the horizon of the point T, which is perpendicular to the vertical CT; and let the ſurface of the atmoſphere ZX be illuminated in P by the rays of the limb of the ſun which is yet under the horizon; and as we ſuppoſe this point P raiſed above the horizon TL, there will appear a part of the arch of the twilight above the horizon. If we draw therefore the *radius* of the earth CP which meets its ſurface at N, and take the arch NI of 8° 36′, we ſhall have the point I, upon the circumference of the earth, where the rays of the upper limb of the ſun, after being refracted in the atmoſphere by entering into it, and in their way, will touch this circumference, and from thence turn themſelves toward P, where they will meet the extremity of the atmoſphere, as has been ſeen in the beginning of this memoir.

But as the ſame thing happens all about the earth; as to the ray IP in the atmoſphere that ſurrounds it, if we draw the *radius* of the earth CI, and thro' the point P the line PV parallel to CI; this line PV will repreſent the circle of the ſurface of the atmoſphere which is illuminated by the rays of the edge of the ſun; it will therefore be this circle PV, which muſt repreſent the arch of the extremity of the twilight ſeen above the horizon from the point T, of which the point P will be the moſt elevated part.

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† Fig. 3.

Let us now conſider the circle PV for the baſe of a cone which has its *vertex* at T, where the eye is placed, the triangle TPV will be the triangle through the axis of this cone, which is perpendicular to its baſe, and its ſurface determines the extremity of the twilight with relation to the ſummit T; we need therefore only conſider the figure of the ſection of this conic ſurface upon a plane.

When one is pretty far diſtant from a plane upon which there is a figure drawn, and cannot know the diſtance of the eye from the extremities of the figure, we always imagine that this figure is in a plane perpendicular to the principal ray which goes out of the eye to the figure, as if it had a circle drawn upon a plane, and this plane was very much inclined to the eye which ſhould look upon the circle; and beſides the plane not being viſible, and this circle being very diſtant from the eye, we ſhall always judge that the figure is elliptic, for we conceive it in a plane perpendicular to the rays drawn from the eye to the figure.

It is the ſame here with the ſection of the cone TPV, of which there can only be a ſmall part above the horizon TL; and as we ſuppoſe that the twilight is in a vertical plane perpendicular to the horizontal line TL, and to the triangle thro' the axis of the cone, we muſt imagine that its figure is upon this plane, and is an hyperbola; ſince the plane parallel to the plane where the ſection is, and which paſſes thro' the *vertex* T of the cone is within. The eye being placed at T, muſt therefore judge that the figure of the arch of the twilight is hyperbolical. But alſo the curvity of an hyperbolical arch being flatter on both ſides its *ver-*

vertex than the curve of an arch of a circle, will appear alſo more flatted toward the horizon, where the refraction will raiſe this arch much more than towards its *vertex*, which makes the difference ſtill greater between the curvity of the twilight, and an arch of a circle.

It is ſeen by this explication, that in proportion as the upper part of the arch of the twilight riſes toward the zenith Z, which is done in a very little time, ſince it muſt run over a quadrant of a circle whilſt the ſun riſes only 8° 36′ toward the horizon, as from the point I toward N in *fig.* 1. that is from the beginning of the appearance of the twilight, and then the part of the horizon occupied by the twilight, is greater than a ſemi-circle, ſince it muſt be determined by the little circle Zy which will make with CZ an angle equal to CPV, which cuts the horizon in t beyond T, where its light hinders the eye at this point T from diſcerning the term of the arch of the twilight, which very readily diſappears from the eye by the rays which come to it from the whole body of the ſun.

It ſtill remains for me to explain how *Kepler* has found the height of the atmoſphere by the twilights to be 10 *German* miles according to the ancients, without having regard to the refraction.

† Let there be the ſurface of the earth IT, its centre C, and the horizon TK, from the point of its ſurface T, if we take the arch TI of its circumference of 18 degrees, and draw the tangent DI to the horizon TK at K, we ſhall have the point K where we ought to begin to ſee the twilight from the point T, for the point K muſt be

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† Fig. 4.

that where the rays of the ſun meet the atmoſphere. It follows therefore, that if we draw CK, which muſt cut in two equal parts in N the arch TI, the angle TCI, or ICK will be 9 degrees, and the triangle ICK will be rectangular in I, and ſuppoſing the radius of the earth CI to be 904 *German* miles, he finds CK to be 914 miles, from which taking away CN of 904, there remains NK of 10 miles for the height of the atmoſphere.

He adds, that this demonſtration neglects all the cauſes except the ſun, as the refraction, the reflection of the rays within the atmoſphere, and the etherial matter which he imagines about the ſun: thus he reaſons from theſe cauſes without applying any calculation to them, to draw from them the height of the atmoſphere of a little above half a mile, which he reckons to be its true value.

He ſays, that the matter of the atmoſphere is homogeneous, and that its ſurface is as well terminated as that of the ocean. It is certain, that the refraction of the rays of the ſun in entering into the atmoſphere, muſt be conſidered as I have done; but he adds that thoſe rays which do not meet the earth, and which in purſuing their way within the atmoſphere, meet its ſurface in going out as if they had entered it, except ſome which are reflected within toward the earth, and which only produce a very faint light, and theſe laſt which are reflected by meeting again the ſurface of the atmoſphere, go out of it alſo; except ſome which reflecting themſelves again within the atmoſphere, go to meet its ſurface in a place which may be ſeen from the earth, which produces in that place the appearance of the twilight. It is eaſy to judge that theſe few rays which ſhould touch the atmoſphere after ſeveral reflections, and ſhould be almoſt all gone out of the atmoſphere, as he himſelf ſaid, could not make impreſſion upon the ſight, and chiefly meeting only the ſurface of the air, which is a very rare body, altho' he ſuppoſes it homogeneous like water.

He afterwards runs out into a great length to prove by ſome calculation which he makes, the neceſſity of the matter which he has imagined about the ſun, which muſt produce the curvity of the twilight; but all this is of no ſervice, and is intirely uſeleſs for this phænomenon, as I have before demonſtrated.

I have already explained why we cannot diſtinguiſh exactly the bounds of the twilight, when it is pretty much elevated above the horizon; and even when it is yet toward the horizon, it very often happens alſo that we cannot ſee it well terminated; for if the firſt rays of the ſun which form it, as dI in the firſt figure, paſs through thick clouds, or vapours, which are upon the ſurface of the earth toward I, they will be turned from one ſide to the other, and do not make in P a light ſtrong enough to ſee there diſtinctly the term of the *crepuſculum*, and this commonly happens. For towards the morning, where the ſun beginning to inlighten ſucceſſively the ſurface of the earth, a great many vapours ariſe; and towards evening, thoſe which had riſen in the day time, fall upon it again by the abſence of the ſun which forſakes them; it will not be the ſame thing if the air is very cold, and without clouds, it is alſo that time alone when the twilight is well terminated in its beginning, or in its end. We muſt alſo add to this, that toward the beginning of twilight, the eye which is at T, ſees the particles of air, which are enlightened very near one another

other, which cauſes a much livelier appearance of light, than when it is raiſed above the horizon, where we only ſee theſe enlightened particles very much ſeparated, which cannot ſtrike the eye ſtrong enough to diſtinguiſh the term of the *crepuſculum*, and much leſs when the enlightened particles are toward the zenith Z.

In fine, if it is not granted that the beginning or end of the twilight appears when the ſun is yet 18° below the horizon, and that it was at 17° or 19°, we muſt only increaſe or diminiſh the height of the atmoſphere, as I have juſt found it about 2000 toiſes for each degree.

Nor do I pretend that the height of the atmoſphere which I have ſuppoſed, muſt be the ſame over all the earth as toward the equator, or towards the poles; but that depends upon obſervations which might be made in thoſe countries; and I am even perſuaded that in the countries towards the poles, the height of the atmoſphere is much greater than in theſe, or I believe greater than toward the equator; but towards the poles the obſervations of it may be very well determined, becauſe of the great cold and clearneſs of the air, which reigns there in winter.

Here is an obſervation which will ſerve to confirm the height of the atmoſphere as I have juſt determined it. In 1676 there appeared in ſome parts of *Italy*, a meteor, which was as bright as the full moon. M. *Montanari*, profeſſor at *Bolonia*, made ſome obſervations on it, and having compared them with thoſe which had been made in other places, he determined the height of this meteor to be 15 mean leagues of *France*, which he printed in a ſmall work, intitled *Fiamma volante*.

We cannot doubt but that all theſe fires or meteors are formed by ſulphureous exhalations which come out of the earth, and becoming inflamed, weigh much leſs than the particles of air of which they occupy the place; but let them be ever ſo light, they are nevertheleſs heavier than the other which we conſider as without any weight. For this reaſon they muſt raiſe themſelves juſt upon the ſurface of the atmoſphere, where they float as long as they laſt; thus the height of theſe fires muſt be the ſame with that of the atmoſphere, and conſequently the 15 leagues of height obſerved of this, which comes to 35,000 toiſes, confirms what I have found for the height of the atmoſphere.

IV. *Of the figure of the earth, by M.* Caſſini.

We ſhall not here undertake to relate the various opinions which have divided philoſophers concerning the figure of the earth.

We cannot imagine any figure which they have not attributed to it; for not to mention thoſe who believed it to be like a column, a drum, a cone, or a tree, the root of which was infinitely extended; ſome have judged it to be flat without admitting any other inequality in it, than what is cauſed by the mountains.

Others being afraid leaſt the waters of the ſea ſhould run out if they were not reſtrained by ſome bounds, gave it the figure of a concave hemiſphere.

Laſtly, others conſidering that the tops of the towers, and high mountains were perceived at a diſtance, while their baſes were concealed below the horizon; that thoſe who were in the moſt elevated places ſaw the ſun riſe earlier, and ſet later, than thoſe who were in the loweſt places; that the ſhadow of the earth appeared in the eclipſes of the moon to have a circular figure; and that thoſe who travelled from north to ſouth, ſaw the ſouthern ſtars riſe above the horizon, whilſt the northern ſtars were depreſſed, judged it to be ſpherical.

This opinion which was founded upon ſolid reaſons, was almoſt generally received by thoſe who undertook before us to determine the magnitude of the earth by geometrical operations. They made uſe of the meaſure of a ſmall portion of its circumference, to conclude by that its whole extent, by ſuppoſing that all the degrees of the meridians of the earth were equal among themſelves; and that the lines perpendicular to the horizon, which paſs through the zenith, and meaſure the degrees in the heavens, were directed towards the ſame point, which they judged to be the centre of the earth.

Several great geometricians of our time have abandoned this hypotheſis of the ſphericalneſs of the earth. Sir *Iſaac Newton* in his *Philoſophiæ Naturalis principia Mathematica*, having conſidered that the force which he calls centrifugal, and which reſults from the daily motion of the earth, muſt raiſe at the equator thoſe parts which endeavour to recede from the axis of the earth; judged that it muſt be depreſſed toward the poles, and he found according to his principles, that ſuppoſing the earth to be of an uniform matter, as denſe at its circumference as towards its centre, the diameter of the equator; muſt be to the diameter which paſſes through the poles as 692 to 689.

M. *Huygens*, in his diſcourſe of the cauſe of gravity, having conſidered that at *Cayenne*, which is only 4 or 5 degrees diſtant from the equator, a pendulum which ſwings ſeconds is ſhorter there than at *Paris* by a line and $\frac{1}{4}$, from whence it follows, that if we take pendulums of equal length, that of *Cayenne* makes the vibrations a little ſlower than that of *Paris*: judged that the cauſe of this phœnomenon could be referred only to the daily motion of the earth, which being greater in each country, according as it approaches nearer the equinoctial line, muſt produce a proportionable effort to reject the bodies from the centre, and conſequently take from them a certain part of their gravity. He adds, that this effort which reſults from the circular motion of the earth, muſt make a plummet, ſuſpended to a cord, vary from the perpendicularity; and as the ſurface of all liquids is ſo diſpoſed, that the line of ſuſpenſion is perpendicular to it, becauſe otherwiſe it might deſcend more, it follows, that the ſea has the figure of a ſpheroid, and that the earth muſt have conformed itſelf to it, when it was collected by the effect of gravity. Upon theſe principles he advances as a paradox, that the earth is not perfectly ſpherical, but of the figure of a ſphere flatted towards the poles, nearly ſuch as an ellipſis would make by turning about its leſſer axis; and he concludes, that the diameter of the equator exceeds the axis of the earth by $\frac{1}{578}$, whereas according to Sir *Iſaac Newton*, this exceſs is but $\frac{1}{230}$ of the diameter of the equator.

But M. *Einſenſcbmid*, a celebrated mathematician of *Strasburg*, having examined the magnitude of the degree, which reſulted from ſeveral dimenſions, made under different parallels by various mathematicians, found that the magnitude of the degree of the earth, drawn from the meaſures of *Snellius* made in *Holland*, was ſmaller than that which M. *Picard* had determined by his obſervations made in *France*, which is more ſouthward; that the magnitude of the degree, which reſults from the meaſures of M. *Picard*, made in the neighbourhood of *Paris*, was ſmaller than that which F. *Riccioli* found at *Bolonia*, which is more ſouthern than *Paris*; and that this was yet ſmaller than the magnitude of the degree, which had been formerly determined by *Eratoſtenes*, between the city of *Alexandria* and that of *Syene*, which was under the tropic of *Cancer*; this in quality of degrees, which increaſed in magnitude, as they approached the equinoctial line, made him judge that the earth was not ſpherical, but had the figure of a ſpheroid prolonged toward the poles, the meridians of which are repreſented by ellipſes, and the equator and parallels by circles.

He determines upon this foundation the inequality of the degrees of the meridian of the circumference of the earth; but he owns that it were to be wiſhed, that ſome of theſe obſervations were again made near the pole, and the equator; to determine more exactly and with more certainty the figure and ſize of the earth; and that the meridian line, which ought to be drawn through the royal obſervatory on each ſide, to the confines of the kingdom, would be of very great importance for the deciding this queſtion.

In his *Treatiſe of the figure of the earth*, he relates alſo the opinions of ſeveral authors, who, as well as he, have judged that the earth was prolonged towards the pole; and he recites among others, Dr. *Burnet*, who conſidering that by the daily revolution of the earth, the maſs of water receives a greater degree of velocity toward the equator, than towards the poles, where it deſcribes ſmaller circles; the parts of water which are moſt agitated, make an effort to remove from the centre of their motion, and cannot raiſe themſelves, becauſe of the air which ſurrounds them on all ſides and reſiſts them; ſo that they are obliged to flow on both ſides to put themſelves *in equilibrio*; for, adds he, the waters which find any obſtacle, flow on the ſide where they find a paſſage, and where their motion is moſt free, ſo that by the diminution of the waters of the ſea, which are toward the equator, the globe of water is a little prolonged towards the poles, and the cruſt of earth which is formed upon it, muſt take the ſame figure.

In this diverſity of opinions concerning the figure of the earth, we have thought it neceſſary to examine what that is which reſults from the obſervations which we have made in the ſouthern part of *France*, compared with thoſe that M. *Picard* had made in more northern places.

M. *Picard*'s meaſure of the earth extends from the parallel of *Amiens*, which is 49° 54′ 46″ to the parallel of *Malvoiſine*, which is 48° 31′ 48″, and in this ſpace, which is about 1 degree and $\frac{1}{3}$, the magnitude of the degree of a meridian amounts to 57060 toiſes.

Our meaſures begin at the royal obſervatory of *Paris*, which is under the parallel of 48050′ 10″, and end at *Collioure*, which is toward the ſouthern extremity of *France*, under the parallel of 42° 31′ 13″. In this extent, which is 6° 18′ 57′, we have found the magnitude of each degree one with another 57100 toiſes. Thus if we ſuppoſe M. *Picard*'s obſervations and ours to be exact in all their circumſtances, the reſult is that the degrees, which are toward the north, are leſs than thoſe which are toward the ſouth; and that conſequently the figure of a meridian of the earth, muſt be ſuch that the degrees ſhould encreaſe the more they approach the equator, and diminiſh on the contrary in going toward the poles; which is the property of an ellipſis, the great diameter of which repreſents the axis of the earth, and the leſſer diameter that of the equator, as will be demonſtrated hereafter.

This ellipſis turning about its great axis, forms by its revolution a ſpheroid, of which the poles are at the extremities of the great axis, and the equator and parallels are repreſented by circles. This figure is what we attribute to the earth, and we ſhall give according to this hypotheſis, a very ſimple method to divide the ellipſes, which repreſent the meridians of the earth divided into degrees and minutes, and determine the inequality of theſe degrees, which are terminated in the heavens by perpendiculars to the horizon, which paſs through the zenith, and cut all of them the axis of the earth in different points. Let * BDCR be an ellipſis which repreſents a meridian of the earth, of which let the poles B and C be at the extremity of the great axis BC, and let the foci E and F be taken at diſcretion.

We would divide this ellipſis into degrees, that is, find ſeveral points H, I, V, ſuch, that the diſtance from the pole to the zenith of each of theſe points be a certain number of given degrees, ſuch as we pleaſe.

Let there be drawn from one of the foci of the ellipſis E, the line ET, which makes, with the axis BC, an angle BET, equal to the diſtance given from the pole to the zenith. Let there be taken with a pair of compaſſes, a ſpace equal to the axis BC; and from the other focus F, as a centre, let there be deſcribed at this interval, an arch of a circle, which cuts in T the line ET; I

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* Plate VI. Fig. 5.

ſay, that the line FT, drawn from the point T to the focus F, will cut the ellipſis at the point H, which is ſuch, that the diſtance from the pole to the zenith of this place, will be of the number of degrees given.

Demonſtration.

From the point H let there be raiſed HZ, perpendicular to the ellipſis, which paſſes through the zenith z, and being prolonged on the inſide, meets the axis of the earth in O, and divides by the property of the ellipſis, the angle EHF in two equal parts. Let there be alſo drawn from the point H, HP parallel to the axis BC, and directed to the pole P, which we ſuppoſe at an infinite diſtance. The angle PHZ or POZ, meaſures the diſtance from the pole to the zenith of an inhabitant, who ſhould be upon the earth at the point H.

By the conſtruction, F T is equal to the axis BC; but BC by the property of the ellipſis, is equal to EH, *plus* HF; taking away the common FH, we ſhall have EH equal to HT. The angles ETH, TEH, will therefore be equal, and conſequently each half of the external angle EHF; but the angle EHO is alſo equal to half the angle EHF; the angles TEH, EHO, will therefore be equal between themſelves, and conſequently the lines ET, HO, will be parallel between themſelves, and the angle POZ, which meaſures the diſtance from the pole to the zenith, from the point H, will be equal to the angle BET, which, by the conſtruction, was made equal to the diſtance given from the pole to the zenith. Q. E. D.

Now if we ſuppoſe the proportion of the grea diameter of the ellipſis BC, to the diſtance EF between the *foci*, ſuch as we pleaſe, we may determine by calculation all the points of the ellipſis as H, which terminate the degrees, making as FT or BC is to EF; ſo is the *ſine* of the angle PET, the given diſtance from the pole to the zenith, to the ſine of the angle ETF or TEH, the value of which will conſequently be known. This angle TEH being added to the angle PET, the given diſtance from the pole to the zenith from the point H, gives the value of the angle BEH; that the line drawn from the focus to the ſought point H, makes with the axis of the ellipſis.

Now in the triangle EHF, of which the ſide EF is known, as well as the angle EHF, which is double of the angle TEH, and the angle FEH ſupplement of the angle BEH; we ſhall have the value of the ſide EH known in parts of the axis BC.

By the ſame method we ſhall find the angles BEI, BEV, *&c.* and the value of the lines EI, EV, for the diſtance from the pole to the zenith, of all the degrees of the circumference of the earth; and in the rectilineal triangles HEI, IEV, of which the ſides HE, EI, EV are known as well as the angles contained between the ſides HEI, IEV, which are the difference between the angles BEH, BEI, BEV determined here above, we ſhall know the value of the chords HI, IV, contained between each degree.

We ſhall therefore have the exact proportion of the chords of each degree of the circumference of the earth in the elliptical hypotheſis. And as the proportion of theſe cords between themſelves is not ſenſibly different from the proportion between the arches of the ellipſes which they ſubtend, we ſhall have at the ſame time the proportion between the degrees of the circumference of the earth at any diſtance from the pole, ſuppoſing the excentricity of the earth to be in a certain quantity.

For a greater exactneſs we might calculate the proportion between the chords of the halves and quarters of a degree of the circumference of the earth, that the difference which might be between the proportion of the arches and that of the chords, was leſs ſenſible; but if we conſider that the exceſs of the arch of a degree upon the chord which ſubtends it, is only about 4 feet; it is eaſy to judge, that the difference between the proportion of the chords of each degree, and that of the arches of the ellipſes which they ſubtend, is abſolutely inſenſible; add to this, that the meaſures which we have uſed for determining the magnitude of the earth, have been made according to right lines, and not according to the curvity of the circumference of the earth.

Having applied the method juſt explained, to the figure of the earth, which we at firſt ſuppoſed to be like the orb of the moon in the elliptical hypotheſis, and of which the diſtance between the *foci*, is, according to the modern aſtronomers, about $\frac{1}{23}$ of the great diameter; we have found that according to this hypotheſis, the degrees increaſe in magnitude, in receding from the pole and approaching to the equator, agreeable to our obſervations, but that this augmentation from one degree to another, at the diſtance from the pole of 40 degrees, was but two toiſes and 4 feet, which is too little to repreſent the inequality of the degrees, which reſults from the compariſon of our obſervations with that of M. *Picard*'s.

We have therefore been obliged to ſuppoſe the excentricity of the earth greater, and we have found, that by eſtabliſhing the diſtance between the foci of the ellipſis, which repreſents a meridian of the earth, double to that which is attributed to the orb of the moon; ſo that this diſtance may be to the great diameter of the earth, as 8,724 to 100,000, that is pretty near as 1 to 11. This ellipſis perfectly repreſents the figure of a meridian of the earth, ſuch as reſults from our obſervations compared to thoſe of M. *Picard*.

We find for example, that the degree contained between the parallels of 49 and 50 degrees, being 57,060 toiſes, according as it has been determined in the meaſure of the earth; that which is between the parallels of 50 and 51 degrees, is 57,071 toiſes, and 2 feet, greater than the preceding by 11 toiſes, 2 feet, and that the ſum of ſix degrees contained between the parallels of the places which we have determined the diſtance of, is 342,600 toiſes, which gives the magnitude of theſe degrees, one with another, 57,100 toiſes, as we have obſerved.

We alſo find, that according to the hypotheſis of the elliptic earth, the ſmalleſt inequality from one degree to another, is toward the poles, and the equator where it is only two or three feet. This inequality in the degrees increaſes afterwards on both ſides to the parallel of 45 degrees where it is the greateſt that is poſſible, and which we have calculated to be about 11 ½ toiſes.

It follows from hence, that the moſt convenient places to know if there be any inequality in the degrees of a meridian of the earth, are contained between the parallels of 40 and 50 degrees, which are exactly thoſe which we have determined by our obſervations. This inequality in the degrees muſt be obſerved toward the parallel of 45 degrees, even tho' we ſhould ſuppoſe the earth to be depreſſed toward the poles; with the difference that the degrees diminiſh in magnitude in approaching the equator, which is contrary to our obſervations.

By continuing this inquiry, we find the magnitude of a degree of a meridian toward the pole to be 56,785 toiſes and ½, and that of a degree near the equator 57,440 toiſes, ſo that from the greateſt to the leaſt degree of the earth, there is 655 toiſes difference.

Taking the ſum of all the degrees of a meridian, we ſhall have its circumference of 20,560,295 toiſes, only 4,295 toiſes greater than the circumference of the earth would be ſuppoſing it ſpherical, when the magnitude of the degree is 57,100 toiſes.

With regard to the axis of the earth, we ſhall find it 6,557,040 toiſes greater than in the ſpherical hypotheſis by 13,856 toiſes, or about ſeven of our leagues. We ſhall have alſo the diſtance between the two *foci* of the earth 286,018 toiſes, or 443 leagues; and in the rectangular triangle DAE, of which the ſide AE, the half of the interval between the two *foci*, is known as well as the hypothenuſe ED, which by the property of ellipſes, is equal to half the great diameter BC, we ſhall find AD to be 3,266,020 toiſes, of which the double DR, the diameter of the equator will be 6,532,040 leſs than the axis BC, by 25,000 toiſes, or 12 or 13 of our leagues.

The difference between the axis of the earth, and the diameter of the equator, will therefore be the $\frac{1}{262}$ of this diameter, greater by half than what M. *Huygens* has determined, and pretty near the ſame with Sir *Iſaac Newton*'s, but contrary ways.

The diameter of the equator being known, we ſhall have its circumference of 20,521,006 toiſes, which gives the magnitude of the degrees of the equator which in this hypotheſis are equal between themſelves, of 57,003 toiſes almoſt the ſame with that of the meridian which is 36 degrees diſtance from the pole.

Taking the difference between the circumference of a meridian which we have found to be 20,560,295 toiſes, and that of the equator which is 20,521,006 toiſes, we ſhall have 39,289 toiſes, or about 20 leagues, by which the circuit of the earth about one of theſe meridians, exceeds its circuit about the equinoctial.

According to theſe principles we might determine in toiſes, or leagues, the diameter and circumference of each parallel; for in the rectangular triangle ELH, the angle LEH, and the hypothenuſe EH, being known, we ſhall find the value of the ſide LH ſemi-diameter of the parallel which paſſes thro' the point H, the latitude of which is known.

The magnitude of the meridians and parallels of the earth being thus determined with regard to our obſervations, they may be uſed in the conſtruction of terreſtrial globes, and geographical maps.

To facilitate the deſcription thereof, we have drawn up a table, in which is marked in toiſes, and feet, the magnitude of all the degrees of the meridians from the poles to the equator.

This table will ſerve to compare not only the obſervations which have already been made at different diſtances from the pole to determine the magnitude of the earth; but alſo thoſe which may hereafter be made with the ſame deſign under ſeveral other parallels.

A TABLE of the degrees of the meridians of the earth.

Height of the pole.	Dist. from the pole to the zenith	Degrees of a meridian.		Height of the pole.	Dist. from the pole to the zenith	Degrees of a meridian.		Height of the pole.	Dist. from the pole to the zenith	Degrees of a meridian.	
D.	D.	*Toises.*	*Feet.*			*Toises.*	*Feet.*			*Toises.*	*Feet.*
90	0	56785	3	60	30	56952	5	30	60	57280	1
89	1	56785	5	59	31	56962	5	29	61	57289	5
88	2	56786	4	58	32	56973	1	28	62	57299	2
87	3	56787	5	57	33	56983	3	27	63	57308	4
86	4	56789	3	56	34	56994	0	26	64	57317	5
85	5	56791	3	55	35	57004	5	25	65	57326	3
84	6	56793	5	54	36	57015	4	24	66	57335	1
83	7	56796	3	53	37	57026	3	23	67	57343	2
82	8	56799	4	52	38	57037	4	22	68	57351	2
81	9	56803	1	51	39	57048	5	21	69	57359	0
80	10	56807	0	50	40	57060	0	20	70	57366	2
79	11	56811	2	49	41	57071	2	19	71	57373	2
78	12	56815	5	48	42	57082	4	18	72	57380	1
77	13	56820	5	47	43	57094	0	17	73	57386	4
76	14	56826	1	46	44	57105	3	16	74	57392	5
75	15	56831	4	45	45	57117	0	15	75	57398	3
74	16	56837	5	44	46	57128	2	14	76	57404	0
73	17	56844	1	43	47	57139	4	13	77	57409	0
72	18	56850	5	42	48	57151	1	12	78	57413	4
71	19	56857	5	41	49	57162	2	11	79	57418	0
70	20	56865	0	40	50	57173	4	10	80	57422	0
69	21	56872	4	39	51	57184	5	9	81	57425	3
68	22	56880	3	38	52	57195	5	8	82	57428	4
67	23	56888	4	37	53	57206	5	7	83	57431	3
66	24	56897	1	36	54	57217	5	6	84	57433	5
65	25	56905	5	35	55	57228	3	5	85	57435	5
64	26	56914	5	34	56	57239	1	4	86	57437	3
63	27	56924	0	33	57	57249	4	3	87	57438	4
62	28	56933	4	32	58	57260	0	2	88	57439	3
61	29	56943	0	31	59	57270	1	1	89	57440	0
60	30			30	60			0	90		

V. *Experiments and reflections upon the prodigious ductility of ſeveral bodies, by M.* de Reaumur; *tranſlated by Mr.* Chambers.

Art has its wonders as well as nature, and the reaſon why we do not perceive them is, that they are always before our eyes. It is enough for us to ſupply our wants, or our luxury, without enquiring into the artifice we are beholden to for it.——Of this we have an inſtance in the wier-drawer's art: the perſon that wears lace or embroidery, ſeldom conſiders the formation of thoſe wonderful threads, but a few philoſophers have examined them, and have drawn excellent proofs from them of the prodigious diviſibility of matter which they might have carried ſtill further, if they had been acquainted with the full effect of the wier-drawer's art. They have even attempted to account for this extreme ductility of metals, though one of the greateſt ſecrets in nature, as depending on that very obſcure property hardneſs, and having its obſcure difficulties beſides.

As to the philoſophy of ductility, we have little to offer more than has already been done, but as to the management and application of it by art, we are in a condition to diſcover ſeveral things, accordingly, having had occaſion to make ſeveral experiments on this head; we ſhall here produce them, and examine the effects of ductility in very different bodies. It will afford no diſagreeable ſpectacle to ſee collected, as into one point of view, all that is remarkable on a ſubject wherein art and nature ſeem to ſtrive which ſhall furniſh moſt curioſities.

Ductile bodies are thoſe, which when beat, preſſed, or drawn, ſtretch without breaking one way, almoſt what they looſe in another: ſuch are metals, which, by hammering, gain in length and breadth, what they looſe in thickneſs, or by being drawn thro' a wier-drawer's iron, become longer in proportion, as their thickneſs is leſs; we have alſo another kind of bodies, which tho' not malleable like metals, may yet be accounted ductile, as glue, gums, reſins, and all bodies which being ſoftened by water, fire, or any other diſſolvent, will draw out into threads. ——— Hence ductile bodies may be divided into 2 claſſes, the firſt whereof comprehends thoſe which are hard and malleable, of which we ſhall treat in the firſt place; the ſecond conſiſts of the ſoft bodies, which tho' not malleable, are capable of being drawn out.

The common method of extending the firſt kind is, by beating them with a hammer: thus it is the artificers in gold, ſilver, copper, and pewter, reduce maſſes of thoſe metals into what figure they pleaſe; but tho' this kind of manufacture deſerves more attention than is commonly beſtowed on it, we ſhall not dwell thereon, our deſign being only to conſider ductile bodies with regard to the great extenſion they are capable off.

The gold beaters, by bare means of a hammer, reduce that metal into plates inconceivably thin: thus it is they prepare thoſe leaves commonly uſed in guilding, which all come from a thick ingot, gradually made thinner and thinner, till at length the leaves become light enough to be blown away by the ſlendereſt puff: to learn in ſome ſurer way, than by the accounts of the workmen, to which *Rohault* confided, how far gold may actually be ſtretched in this method, I took a quantity of the thinneſt leaves, ſuch as thoſe in their common books, and after meaſuring them carefully, weighed them in a fine ballance, by which means I found, that a grain of gold was here beaten into an extent of 36 ſquare inches $\frac{1}{2}$, and 24 ſquare lines, on which footing, an ounce of gold, which, in a cubic form would only have meaſured 5 lines $\frac{1}{3}$, either in breadth, length, or depth, and which would only cover a ſurface of about 27 ſquare lines, when ſtretched by the gold-beater, will cover upwards of 146 ſquare feet $\frac{1}{2}$, a degree of extenſion greater by almoſt $\frac{1}{2}$, than they were able to give leaf-gold 90 years ago. It was looked on with ſurprize in father *Merſenne*'s time, that an ounce of gold might be made into 1600 leaves, which altogether, only covered 105 ſquare feet.——It would be tedious here to explain how the art arrived at its preſent perfection. We ſhall not ſo much as mention the ingenuity of the operators, who, from bullocks gutts have procured that delicate parchment without which gold could not be reduced into leaves ſo thin.

After all, how conſiderable ſoever the extenſion of gold may be in the leaf, it will appear but a ſmall matter, when compared with that which the gold wier-drawers give it. There are ſome leaves of beaten gold, which in certain places are not $\frac{1}{30000}$ of a line thick; but $\frac{1}{30000}$ of a line is a conſiderable thickneſs, when compared with the gold which covers the ſilver wier, commonly ſpun upon ſilk.

To conceive the extent to which gold is then ſtretched, and how prodigiouſly thin it muſt be, it will be neceſſary to have a general idea of the gold-beater's art.——What we call gold wier, every body knows is only ſilver wier gilt, and is drawn from a thick bar of ſilver; they take ſuch bar, weighing about 30*lb.* and rounding it into the form of a cylinder or roller, about 15 lines in diameter, and 22 inches long; they gild it with leaves prepared by the gold-beaters, the leaves however employed for this purpoſe are thicker than thoſe intended for common gilding, and they frequently apply ſeveral, one over another, but tho' the layer of gold be here thicker than in other gildings, yet is it very thin, as may eaſily be inferred from the quantity of gold therein. To gild theſe 30 *lb.* of ſilver, they never uſe above ſix ounces of gold, which is enough even to make a ſuper-gilding; but the common proportion is only 2 ounces; and they frequently ſcarce allow one, where the wier is to be but ſlightly gilt, as in the common gold wier of *Lyons.* Upon the whole, the layer of gold, which covers the ingot, is never above $\frac{1}{15}$ of a line thick, frequently only a 30th or 45th, and ſometimes barely a 90th part.

Yet how much thinner muſt this thin layer of gold be ſtill brought, and how many times divided and ſubdivided! The ingot itſelf cover'd with it, muſt be drawn to a great degree of fineneſs, equal, or even ſuperior to that of a hair; this is done by paſſing it ſucceſſively thro' ſeveral holes, each finer than the other, and in proportion as its diameter is reduced by the ſlenderneſs of the hole, its length is increaſed, and conſequently its ſurface augmented, yet the gold, which covers the ſilver ingot, ſtill continues to cover it, how prodigiouſly ſoever extended, nor ever leaves the ſmalleſt point bare. Now how many diviſions muſt it have undergone, when the ingot is reduced into a thread, whoſe diameter is 9000 times ſmaller than that of the ingot.

To give a further idea of the prodigious ductility of gold, I weighed $\frac{1}{2}$ a drachm of the fineſt wier, and meaſuring it with care, found it 202 feet, conſequently the ounce of wier was equal to 3232 feet, and the pound, or 12 ounces, to 38784; the ingot therefore, which weighed 30*lb.* and was only 22 inches long, had now arrived at the length of 1163520 feet; ſo that reducing the feet into fathoms, and taking the league of 2000 fathoms, the 22 inches length will be found extended into 96 leagues and 196 fathoms, an extenſion vaſtly ſuperior to what father *Merſenne* and *Furetier* mention; the latter of thoſe gentlemen obſerves after the former, that $\frac{1}{2}$ an ounce of wier will reach 100 fathoms and more; this more is very conſiderable, ſince 100 fathoms, for $\frac{1}{2}$ an ounce, would only be 1200 feet for an ounce, whereas we have found it 3232 feet. *Rohault* alſo comes far behind us in the extenſion of gold.

But the ingot, tho' reduced into ſo ſlender a wier, is not left here, but remains to be lengthened ſtill further; the greater part of the gold wier is ſpun upon ſilk, and before ſpinning muſt be flatted, by paſſing it between 2 wheels of poliſhed ſteel; and theſe wheels in thus flatting it, draw it out $\frac{1}{7}$ more, ſo that the ingot is now arrived at 111 leagues long; theſe leaves of flatted wire are very ſlender, their breadth is only about $\frac{1}{8}$ of a line, from whence it follows, that their thickneſs is only $\frac{1}{250}$ of a line, as appears by an eaſy calculus. For the weight of a cubic foot of gold, and a cubic foot of ſilver, being known by accurate experiments; we ſuppoſe here that the cubic foot of gold weighs 21220 ounces, and the cubic foot of ſilver 11523, we ſhall not ſtand to ſhew the method whereby it appears that theſe ſilver laminæ are only $\frac{1}{256}$ of a line thick: but inſtead thereof turn our eyes upon the extream thinneſs of the leaf gold which covers them; this will appear truly amazing, if we conſider the ſmall quantity of gold applied on the ſilver ingot; ſuppoſe 2 ounces, as we have ſhewn that leſs is often us'd by calculation, it will appear, that the ſurface cover'd by theſe two ounces of gold, is 2380 ſquare feet, or that an ounce of gold covers 1190 ſquare feet, whereas the gold beaters can only extend it to 146 ſquare feet.

Now how prodigiouſly thin muſt the gold thus extended be, by the preceding calculus, we ſhall find its thickneſs only $\frac{1}{175000}$ of a line, and yet to have the gold which covers the ſilver, this $\frac{1}{175000}$, the gold muſt be ſuppoſed equally thick in every part, which yet is contrary to all experience, for how carefully ſoever the leaf gold be beat, 'tis impoſſible not to leave inequalities in it, 'tis evident from their greater and leſs tranſparency, that they are at leaſt twice as thick in ſome places than in others, and conſequently in gilding the ingot, they will cover it very unequally, leaving the gold thicker by half in certain places than in others; now if we enquire, the thickneſs of the gold in thoſe parts where it is thinneſt, it will be only found equal to the 262,500th part of a line.

Yet, is not this the utmoſt bound to which the ductility of gold may be carried; in lieu of 2 ounces, 1 need only have been uſed, in which caſe, the gold which covers the ſilver wire, would only have been in ſome places the 525,000th part of a line; but what is more, this ſilver-wire for all its thinneſs might be made thinner by $\frac{1}{2}$, yet ſtill remain gilt. All required hereto, is to ſqueeze it cloſer between the wheels, till the propoſed extenſion be effected, and the wire meaſure $\frac{1}{4}$th of a line broad; here therefore the gold which covers them will be reduced to the 1,000,000th part of a line thick, in which caſe, their thickneſs makes a leſs part of a line, than a line does of 1,200 fathoms.

It may perhaps be ſurmized, that the gold is much thicker than appears by this calculus, ſince it may be divided into little ſeparate grains, yet placed ſo near each other, as to give its colour to the ſilver. In fine, it is very natural to ſuppoſe, that the gold which covered the flatted wire does not form a continued leaf; but experience ſhews the contrary; for upon diſſolving ſome of the gilt threads in aqua fortis, how ſmall or thin ſoever they may be when the menſtruum has had its effect, they will be found turned into ſo many ſmall hollow tubes, the aqua fortis having eat out the ſilver, but left the gold intire; whence it appears, that the gold which covered the ſilver, forms one continued body; and conſequently we are able by art to divide a piece of gold, a line thick, into a million different leaves.

The art of extending ſoft bodies has not yet been carried near ſo far. The only body of this kind that we can ſtretch any thing conſiderably, is glaſs; and it will appear extreamly ſurpriſing that a ſubſtance, which of all others, is the moſt brittle, and ſtubborn, ſhould be laid down as the firſt among ſoft ductile ones; 'tis known indeed, that when fuſed by the fire, the workman can faſhion it like ſoft wax; but what is moſt remarkable, and nearlieſt concerns our preſent ſubject is, that it may be drawn out into threads inconceivably ſlender, a common ſpinner does not form her threads of flax or hemp with half ſo much facility, as a glaſs-man will make threads of this brittle matter.

Few people but have ſeen a ſort of plumes uſed in the toppings of childrens caps, and in other ornaments, which 'tis well known, are no other than bunches of glaſs threads, tho' finer than any hairs, and bending like them, by the ſmalleſt breath of wind. A manufacture ſo very extraordinary, would doubtleſs bear a very high price, were it not ſo very ſimple, and eaſy to be performed; it employs 2 perſons at the ſame time, but ſcarce requires any ſkill or dexterity in either of them.

The 1ſt holds one end of a piece of glaſs over the flame of a lamp, and when it is ſufficiently ſoftened, the other applying one end of a glaſs hook thereto, draws it away again without ſeparating it from the reſt of the maſs, then fitting this hook upon the circumference of a vertical wheel, about 2 feet $\frac{1}{2}$ in diameter, not unlike a common ſpinning wheel, nothing remains but to turn it about; for in proportion as it turns, it draws part of the melted glaſs towards it, which part ſtill adhering to thoſe before and after them, form a thread which winds round the rim of the wheel; each revolution lays on a new round of thread, till at length the circumference of the wheel is covered as it were with a skain of glaſs thread; the melted maſs diminiſhing apace, and winds out like a clue on the rim of a wheel; the parts as they recede from the lamp, grow cool, and ſtick faſter to each other; the reſt which are nearer the fire remaining looſe: and thus continually obeying the attraction of the former. Nor is the workman obliged to turn his wheel ſlowly, for fear of breaking the thread; he may turn as ſwift, and diſpatch his work as faſt as he pleaſes, without the thread breaking at all the oftener.

The threads thus formed, are not uniformly round, but their circumference is a very flat oval, being twice or thrice broader than thick; ſome of them are extremely fine, ſcarce ſhort of the thread of a ſilk worm; and accordingly theſe are flexible to a ſurpriſing degree. One may knot the 2 ends of ſuch a thread, and draw them till the hole in the middle be ſcarce $\frac{1}{4}$th of a line in diameter e'er it will break.

How ſtiff ſoever, and inflexible glaſs appears in the lump, it is not eſſentially ſo: if we had the art of drawing much finer threads, they would be more flexible in proportion; whence it may be concluded, that if we could draw threads as ſlender as thoſe wherein the ſpiders lay their eggs, we might make glaſs threads proper to be wove in ſtuffs; and that if glaſs be not malleable, it may however (if I may uſe the term) be texable. I have tried ſeveral ways for making glaſs threads vaſtly finer than is commonly done, but have not been able to get them to the neceſſary length. The danger is in giving too great a degree of fuſion to a matter ſo very thin, as that which muſt here be uſed, not to mention the difficulty of drawing it gently enough, and in an equable manner. The method which ſucceeded the beſt is, that which follows; I took a glaſs thread ſeven or eight inches long, and ſuſpending it in the air by one end, I loaded the other end with a piece of wax hardly weighing a tenth part of a grain, which yet was enough to draw the glaſs thread downwards. Near the thread thus ſuſpended, I drew a little taper; upon the arrival whereof, at a certain degree of nearneſs, I could perceive the little weight deſcend in jerks:

by this means I frequently ſtretched a piece of thread $\frac{1}{4}$th of an inch long, to the extent of nine or ten inches, but have rarely been able to go much further, by reaſon the ſlighteſt breath of wind would blow the flame of the taper ſo near the thread, as to melt and break it; it was not eaſy to make a ſufficient number of threads in this manner, whereof to compoſe one pretty thick one; but by the experiment I learned, that one may make threads of glaſs finer than thoſe of any ſilk-worm. Thoſe I drew in this manner ſeemed almoſt as fine as a ſpider's threads. I would willingly have learned to what degree they were flexible, for they appeared to be prodigiouſly ſo; but they were too fine, and ſhort, and their number too ſmall for this purpoſe.

Thus much is certain, that the matter whereof, even the ſpiders and ſilk-worms ſpin their threads, is brittle in the maſs, like all dry gums, as I have tried, by letting it dry; and it may be added, that tho' the threads ſpun hereof were leſs flexible than they are, they might nevertheleſs be woven, whence it ſeems to follow, that we only want the art of extending the glaſs, to be able to work it into cloth.——In effect, if the glaſs threads were fine enough to give them the flexibility neceſſary for weaving, they would naturally be ſtrong enough to prove their ſtrength. I hung ſeveral weights to the fineſt glaſs threads, which the workmen make, and found that one ſingle thread would ſuſtain 15 drachms, or near 2 ounces without breaking; 'tis true theſe threads were 3 or 4 times broader than a thread of a ſilk-worm, but they did not ſeem any thicker, whence it ſeems to follow, that they would be conſiderably ſtronger than the threads of a ſilk-worm, even though they were equally ſlender, ſince one of the ſtrongeſt threads of ſilk will only ſuſtain 2 drachms $\frac{1}{2}$, whoſe ſtrength of conſequence is only $\frac{1}{6}$ of that of a glaſs thread. A diſproportion much greater than that between their ſolid contents. Accordingly picking a large bunch of the fineſt of theſe threads, and dividing them into ſeveral portions, and twiſting theſe together as barbers plait treſſes of hair, we find them very ſtrong, not but ſeveral other threads will break in the plaiting; and after all, there is no likelyhood that any great advantage will ever be procur'd from glaſs threads.

Gums, reſins and wax, are alſo ductile ſoft bodies, but none of them are much conſidered by artificers in that capacity; 'tis true the wax-chandler's draw their torches or tapers through holes, like thoſe of a wire-drawer, but this is not to extend, but to make them round and ſmooth.

If we be defective in the management of the claſs of ductile ſoft bodies, 'tis in ſome meaſure compenſated by that artifice found in ſeveral animals, for extending it beyond all imagination; ſo that we have nothing to do but employ the threads prepared for us. 'Tis obvious we here ſpeak of the ſilk-worm's threads which are formed of a viſcous matter iſſuing from the body of the inſect, which arrives at a conſiſtence much as glaſs threads become hard, by receding from the lamp, tho' from a different cauſe, as ſhall be ſhewn hereafter, nor ſhall our enquiry into the extenſion of ductile ſoft bodies, terminate in the ſilk of worms. Theſe animals which yield the moſt profit, rarely afford us the moſt curioſity, but nature on the contrary, ſeems to have beſtowed moſt art in the contrivance of thoſe which are troubleſome to us, and for which we even have an averſion; thus ſpiders, thoſe miſchievous inſects, which probably would not ſpin at all, unleſs to incommode us, as we have already ſhewn, in an enquiry into the produce of their ſilk, in the memoirs for the year 1710, are incomparably better fitted than ſilk-worms, to ſhew how far nature can go in extending a viſcid juice.——In the diſcourſe already cited, we only ſpoke occaſionally of the extream fineneſs of their threads, to prevent digreſſing too far from the principal point, we ſhall now examine a little nearer, and inſpect that admirable mechaniſm whereby they are formed.

The illuſtrious *Malpighi* in his anatomy of the ſilk-worm, deſcribes the parts from whence their ſilk is derived, and we ſhall find another *apparatus* in the body of ſpiders——near the ſpiders tail there are ſix teats or *papillæ* *, the end of each whereof is a ſpinning inſtrument, through which the threads are drawn; theſe moulds or ſpinning inſtruments are inconceivably ſmall, in a compaſs leſs than a pin's head. There are enough of them to give riſe to a ſurpriſing multitude of threads, as may be eaſily perceived by their effects; taking a large garden ſpider ready to lay its eggs, and applying a finger on one of its papillæ, as we withdraw the finger, a ſurpriſing quantity of ſeparate threads will come after it, as ſhewn by KAMN, &c. I have endeavoured to learn their number by uſing a good microſcope, and have frequently told upwards of 70 or 80, but could eaſily perceive that they were infinitely more, tho' they all aroſe from a ſmall part of the papillæ. In effect, if I were to ſay that the end of each *papilla* furniſhes a thouſand threads, the number would perhaps be thought exceſſive, and it appears to me much too ſmall to expreſs the number of theſe threads, as every body may be convinced, by examining a *papilla* of a chamber-ſpider with a microſcope. In this contemptible inſect we find a beautiful ſtructure; the end of the *papilla* is divided into a multitude of ſmaller eminences, much like flies or butterflies eyes, and each eminence doubtleſs ſerves to make a different thread, or the ſeveral dents between the eminences may perhaps be pierced, and thus give paſſage to ſo many threads, the little eminences only ſerving to keep the threads from mixing together; theſe eminences are not ſo ſenſible as the papillæ of garden ſpiders, but in lieu thereof we find a foreſt of little hairs, which may probably anſwer the ſame purpoſes—be this as it will, it appears inconteſtable, that from each papilla of a ſpider, there may ariſe threads from above a thouſand different places, and conſequently the animal having 6 *papillæ*, may give paſſage to 6000 threads; nor is the artifice of nature barely ſeen in piercing ſuch immenſely ſmall holes, but the threads are already formed when they arrive at the *papillæ*, each of them having its little canal or vagina apart; in effect, we find them formed and ſeparate from each other at a great diſtance from the *papillæ*, and to explain the whole mechaniſm, we muſt go back as far as the ſource of the juice they are compoſed of.

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* Plate VI. Fig. 9.

In inſects ſo ſmall and ſoft ſuch delicate parts are hardly diſtinguiſhable without a little attention, but boiling the animal, or drying it, or leaving it a few hours in ſpirit of wine, renders all the eſſential parts viſible without a microſcope. Near the riſe of the belly, as at DD *, we find two little ſoft bodies, which are the firſt ſources of the ſilk in figure and tranſparency; they reſemble glaſs drops, whence for the conveniency of expreſſion, we ſhall not ſcruple to call them drops *; the point of each drop goes winding as in R, and forms a multitude of bends towards the *papillæ*. From the baſe of the drop proceeds another branch much bigger than the former, as S, which making alſo a greater number of folds, forms ſeveral knots, and advances like the other towards the hind part of the ſpider.

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* Fig. 7.

I have ſometimes unfolded this laſt branch till it were 9 or 10 inches long, tho' only a part of it were then unfolded. The drop and branches which ariſe from them, contain the matter whereof ſilk is to be formed; but this matter is yet ſoft, and unleſs the ſpider have been dried, will hardly draw out into long threads. The body of the drop may be conſidered as a reſervoir, and the 2 branches as canals proceeding from them. If the ſpider be not over-boiled, the branches will be viſibly ſeen inveſted with a membrane which prevents the tranſparency of the liquor from appearing, but by rubbing the canal gently, this membrane comes off a little further; behind, there are 2 ſmaller drops which ſend forth one more branch from their point, ſo that on each ſide the ſpider there are 3 drops which ſend forth their liquor, by 3 canals, to the proper reſervoirs from whence it is immediately ſupplied for the formation of ſilk.

On each ſide the ſpider, as at EE †, there are three other bodies, which may be conſidered as the ultimate reſervoirs wherein the liquor is gathered; theſe we call the grand reſervoirs ‖; they are much bigger than the drops, and the three which are on the ſame ſide are ranged in ſuch manner by each other, that they only ſeem

* Fig. 10. † Fig. 7. ‖ Fig. 11.

to form a ſingle body, they are very different in figure, and yet have this in common, that each of them makes 6 or 7 bends, and are pretty equably thick throughout, tho' one of their extremities is bigger than the other, the larger end as VVV, is next the head of the inſect, and the ſmaller TTT, next the *anus*; theſe latter terminate in a point, and almoſt meet each other, like the three middle fingers of the hand. From the three points of theſe reſervoirs proceed the threads, or at leaſt the chief parts of the threads, which iſſue at the three *papillæ*, each reſervoir being deſtined to ſupply one *papilla*; this is not difficult to perceive, for not only the point of each of theſe reſervoirs is ſeen terminating in a thread; but if the adjacent parts be dexterouſly managed, we perceive a multitude of threads which proceed from theſe bodies, and may be traced to the *papillæ*.

In fine, at the origin of the *papillæ* we diſcover ſeveral fleſhy tubes, and have reaſon to imagine, that there are as many *papillæ*. If the membrane or pellicle which ſeems to cover theſe tubes be removed, we find them full on the inſide, with threads all diſtinct from each other, and which of conſequence, under one common cover, had each of them their particular one.

'Tis true, upon tracing theſe threads we find ſeveral of them which come further off than the points of the grand reſervoirs, ſome of them ſeem to come from the middle, others a little lower, and others a little higher; ſo that the whole multitude of threads collected near the papillæ of the ſpider do not all ſeem to ariſe from the points of the reſervoirs; 'tis more probable that ſome ariſe from the ſeveral angles, or perhaps from other parts of the reſervoirs. All we can affirm for certain is, that theſe bodies ſeem to have one common tegument, and that there are a multitude of threads which follow all their windings.

As to the manner wherein this juice is gathered in the drops, and its paſſage thence into the grand reſervoirs, the eye can give us no information. *Malpighi*, as clear ſighted as he was, contented himſelf with deſcribing the veſſel wherein the liquor of his ſilk-worm is collected, without either explaining the manner wherein it comes thither, or its paſſage out again. What then can be expected from us, in an inſect much ſmaller than the ſilk-worm, and which contains 6 or 7 thouſand times more parts? 'Twill be enough for us to make ſome reflections on the prodigious ductility of the matter whereof their threads are compoſed, and the immenſe fineneſs of the holes, thro' which they paſs, and the tubes they are moulded in.

We have obſerved, that from the end of each *papilla* there may iſſue upwards of a thouſand threads, yet this end of a papilla is no broader than a pin's head, and the holes are of neceſſity ſeparated from each other by intervals which muſt be greater by much than the holes themſelves; and this even in the largeſt ſpiders: for if we conſider the young ones produced by theſe, we ſhall find them to ſpin threads the moment they quit their neſt; theſe threads 'tis true are hardly to be perceived; but the webs formed of them are ſufficiently viſible, being frequently as thick and as cloſe as that of a chamber-ſpider; and this by reaſon 4 or 5 hundred young ſpiders concur to the ſame work. How ſmall now muſt the holes be thro' which theſe threads are drawn? The imagination can hardly reach the ſmallneſs of one of theſe papillæ. The whole ſpider perhaps is ſcarce ſo big as a ſingle papilla of one of the parent ſpiders, as may be eaſily ſhewn; for every pregnant ſpider lays 4 or 5 hundred eggs, which are all incloſed in a ball; and as ſoon as a young ſpider has broke thro' this ball, they begin to ſpin. How ſlender then muſt each of the threads be that iſſue from each papilla! It were perhaps needleſs to ſhew, that nature can carry the ductility of this matter ſtill further, otherwiſe it might eaſily be done; for ſome ſpiders are ſo ſmall at their birth, that there is no diſtinguiſhing them without a microſcope. They are of a red colour, and being gathered in large crowds, ſeem only to the naked eye, like cluſters of red ſpecks; and yet all of them, how imperceptible ſoever, make webs, and conſequently ſpin. What then muſt the tenuity be of the threads which iſſue from the holes of their papillæ? A hair compared to one of theſe threads muſt be thicker than the largeſt bar or ingot compared to the ſlendereſt ſilver-wire. To ſay no more, theſe threads, which however are conſiſtent, and hang together, are leſs in diameter than the thinneſt layer of gold which covers the fineſt ſilver-wire.

We have already obſerved, that the matter whereof theſe threads are formed, is a viſcous juice; its firſt appearance is in the drops above-mentioned, where it has but little conſiſtence; but we find it much improved in the grand reſervoirs, to which it is tranſmitted from the drops. We ſuppoſe a conſiſtence required in the way by a ſecretion of part of the moiſture, or aqueous liquor mixed therewith, made by parts deſtined for that purpoſe; but the liquor dries ſtill further in its paſſage to the papillæ, thro' proper canals, till at length it becomes thread. At its appearance without the hole, it ſtill remains ſoft and glutinous, ſo that the threads of different holes ſtick together at ſome diſtance therefrom, and the matter does not become perfectly dry, till the air has exhaled the reſt of its moiſture.

Thus much is eaſily proved by drying a large ſpider by the fire, or boiling it in water. A little drying or boiling will give the matter in the drops a ſufficient conſiſtence to fit it to be drawn out into threads, and at the ſame time will unfit that in the grand reſervoirs from being drawn; but boiling the ſpider to a further degree, the matter in the drops becomes a kind of hard glue, incapable of any further drawing; whence it appears evident, that it is by drying or evaporating the uſeleſs moiſture, that the matter of ſilk becomes ſilk.

I was well nigh betrayed indeed by a ſpecious experiment into an opinion, that it is not by the evaporation of an aqueous matter, that the threads of ſilk acquire their conſiſtence. Drawing out ſome threads from the hind part of a ſpider, and winding them on a ſtick, I plunged the ſpider, ſtick and all, in water, and continuing to turn the ſtick about, found the ſilk threads continue to iſſue from the papillæ as before.——I was not yet in the ſecret of the method of the ſpider's ſpinning, nor knew that the threads had acquired a conſiſtence before they iſſued from the holes: this conſiſtence, tis true, is yet defective, but ſuffices to make them hang together; and they continue to dry in the water. A proof hereof is, that if the drops, or the grand reſervoirs, be ſteeped in cold water, the contained matter gains no conſiſtence thereby; on the contrary, if the matter, either of the drops, or the grand reſervoirs, be left a while in ſpirit of wine, it will acquire the ſame conſiſtence, as if the ſpider had been dried.

We may add, that the matter in theſe reſervoirs reſembles ſilk in its colour, tho' in no other quality. 'Tis much like a gum, or a tranſparent glue, and breaks when bent to a certain degree, or a certain number of times, like glaſs. It is only flexible when divided into very ſlender threads; and if nature have multiplied the number of holes in the papillæ, 'tis probably becauſe the ſilky matter formed in their bodies, is naturally more brittle than the matter of ſilk-worms; and requires to be divided in ſmaller parts. Without this, what neceſſity was there for forming ſuch a number of ſeparate threads to be united again after, when a ſingle canal might have made a thread big enough at once?

'Tis probable, that the matter in the reſervoirs never grows perfectly dry, even in the air; but that the middle parts ſtill remain a little moiſt; the external ſurface muſt neceſſarily dry the firſt; and this ſurface, when dried, is indiſſoluble by water, and impenetrable by it; and conſequently muſt prevent their moiſture in the middle of the maſs from eſcaping, as well as the external moiſture from entering; and this probably was one of the reaſons why ſpiders threads are naturally ſo ſmall, ſince to make them ſtrong they muſt be very dry; and they never would have been ſufficiently dry if they had been leſs ſlender.

It would now be time to explain from whence the prodigious ductility of this matter ariſes, but it is rather time to deſiſt, the length of our memoir being but ill attoned for by cloſing the recital of certain facts without reaſonings, which are always precarious.

An explanation of the figures in plate IV.

Fig. 6. is one of the ſpecies of ſpiders, which ſpin ſilk, ſeen at the upper part.

Fig. 7. the ſame ſpider ſeen underneath. AA mark the place where the belly is joined to the breaſt.

At B are the *papillæ* and the *anus.*

DD ſhew the place, where the firſt reſervoirs, which we call drops, are placed.

EE ſhew the place where the laſt reſervoirs are.

Fig. 8. is a part of the belly of a ſpider magnified.

F is the *anus.*

GGGG are the 4 great *papillæ*, viſible only when the ſpider holds them as in this figure, which it commonly does when one holds it.

Fig. 9. is alſo a part of the belly of a ſpider magnified; it ſhews all the *papillæ* diſpoſed as they are when one preſſes the belly of the inſect.

HH are the two moſt diſtant *papillæ* from the *anus.* A bundle of threads *a* comes out of one of theſe *papillæ.*

II are two great *papillæ*, almoſt equal to the former.

KK are two ſmaller *papillæ.*

L is a fleſhy part which covers the backſide of the ſpider, it raiſes it up, to let the excrements go out.

OMP are 3 *papillæ* magnified; different ſpecies of ſpiders have them differently made.

O is one of the *papillæ* H.

M is one of the *papillæ* I. It appears that a prodigious number of ſeparate threads N proceed from this.

P is one of the middle *papillæ* K; all the ſorts have a quantity of hairs; but it would have made them too confuſed, if we had put all that the microſcope diſcovers.

Fig. 10. RQS is one of the reſervoirs which we have called drops; the part Q is the neareſt to AA in *fig.* 7. R is part of the drop ending in a point. S is that which makes a net-work, before it arrives at the great reſervoirs.

Fig. 11. are 3 of the great reſervoirs placed one above another, but not quite ſo near as they are naturally. Our view was to ſhew that they are three. In the animal they ſeem at firſt ſight to make but one body. VVV are the neareſt ends to the head of the inſect.

TTT are the fineſt ends, and the neareſt to the *anus*.

Fig. 12. is one of theſe reſervoirs ſeen ſeparately.

VI. *Reflections on the obſervations of the tides by M.* Caſſini*.

Philoſophers are not yet agreed as to the cauſe and effects of the flux and reflux of the ſea.

Poſſidonius, according to *Strabo*, ſays, that the motion of the ſea imitates the revolution of the heavenly bodies, and that there is in the flux of the ſea a daily motion, another which follows the revolution of the lunar months, and an annual one.

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* Aug. 2, 1713.

That the diurnal motion is that which the ſea makes in riſing and falling twice in the day, that the monthly motion ſhews itſelf by the different heights of the tides, which are great toward the new moons, diminiſh to the firſt quarter, and increaſe to the full moon, after which they fall again. As for the annual motion, he had learned from the inhabitants of *Cadiz*, that toward the ſummer ſolſtices the fluxes and refluxes of the ſea were the greateſt, which made him conjecture, that they diminiſh to the autumnal equinox; that they afterwards increaſe to the winter ſolſtice, after which they diminiſh, and ſo on ſucceſſively.

Pliny affirms, that the ſun and moon are the cauſe of the flux and reflux; he ſeems to be of the ſame opinion with *Poſſidonius*, as to the daily motion of the flux and reflux of the ſea, and in that which is obſerved in each revolution of the moon; but he affirms on the contrary, that the greateſt tides happen in the equinoxes, and the ſmalleſt in the ſolſtices; and that they are alſo greater in the autumnal equinox, than in the *vernal*. He adds, that the tides are leſs when the moon is northern, and diſtant from the earth, than when it is ſouthern, and its force acts nearer; and that in the ſpace of eight years, after 100 revolutions of the moon, the ſame principles of the motion of the tides, and the ſame augmentations are obſerved. He obſerves laſtly, that all theſe changes do not happen exactly in the times here above marked, but ſome days after. The effects of things which paſs in the heavens are not felt upon the earth ſo ſoon as they are ſeen.

Several modern philoſophers have alſo diſcovered three ſorts of motions in the flux and reflux of the ſea; one which it makes twice every day; another which follows the periods of the moon; and the third which is ſeen every year at the time of the equinoxes and ſolſtices.

They all agree with *Pliny* in ſuppoſing that the tides are greater in the equinoxes than in the ſolſtices; but they differ among themſelves in what concerns the cauſe of theſe *phænomena*.

Galileo is of opinion, that the principal cauſe of the flux and reflux of the ſea, proceeds from the motion of the earth about its axis, which it makes in 24 hours, during which it is carried on at the ſame time by its annual motion, which it makes about the ſun in the ſpace of a year. Altho' both theſe motions are made from the weſt to the eaſt, each point of the ſurface of the earth muſt have different degrees of velocity with relation to a fixed point taken in the heavens. For by the daily revolution of the earth, the parts expoſed to the ſun are carried off a different way to that by which the earth is moved by its own motion; and, on the contrary, the parts of the ſurface of the earth which are in the oppoſite hemiſphere to the ſun, are carried off by the daily revolution the ſame way by which they are carried on by its own motion; from whence there reſults a compound motion, the velocity of which is greater than in the preceding caſe, and varies according to the different directions of theſe two motions. The parts of the ſurface of the earth being therefore moved ſometimes ſlower, and ſometimes faſter, in the ſpace of 24 hours, it follows, that the waters contained in the ſea, which cannot exactly follow the motion of the ſurface of the earth, are obliged to flow and ebb in the ſpace of a day, juſt as water contained in a veſſel would do, which being carried away with a certain degree of velocity, on a certain ſide, would flow back on the oppoſite ſide, and afterwards return toward the other edge when this velocity ſhould begin to abate conſiderably. From hence he concludes, that there muſt be in it a flux and reflux in the ſpace of 24 hours; but becauſe of the water which always tends to be in *equilibrio*, and ſeveral accidents which may happen, as the different depths of the ſea, and of the direction of the coaſts of the ſea which interrupts its motion, the flux may accelerate 2, 3, 4, 5, or 6 hours, which makes them commonly obſerve in the *Mediterranean* the flux of the ſea every ſix hours, though in other places we may find it different.

With regard to the motion of the tides which follow the periods of the lunar months, he pretends that they are produced by the inequality of the motion of the earth, which acquires a greater degree of velocity when the moon is in conjunction than when it is in oppoſition. For this he ſuppoſes, that the force proceeding from the ſun acts in like manner at an equal diſtance from it, and moves with more velocity the bodies which are nearer the centre of their motion, than thoſe which are farther diſtant; from whence it follows, that the moon muſt have a greater degree of velocity in its conjunction, when it is nearer the ſun, than in its oppoſition, when it is farther diſtant from it, which alſo contributes to accelerate or retard the motion of the earth, like a pendulum, which makes quicker or ſlower vibrations, according as the plummet is placed nearer or farther from the centre of its motion. This inequality of the motion of the earth in the conjunctions and oppoſitions, the period of which is the ſame as that of the revolution of the moon, is the cauſe, as he imagines, of the inequalities that are obſerved in the tides in the courſe of a month.

As for the inequalities which are obſerved in the tides in the courſe of the year, *Galileo* imagines that they proceed from the difference which reſults from the compoſition of the annual and daily motion, according to the different ſituations of the earth upon the ecliptic. For the daily revolution being made about the poles of the equator, and the annual revolution about the poles of the ecliptic, which is 23 degrees and ½ diſtant from it, it follows that when the earth is in its tropicks, the circle of the declination concurs with the circle of a latitude which is called colure, and both theſe revolutions are made in the ſame direction; whereas, when the earth is in the equinox, theſe motions are inclined to one another in the direction of 23 degrees and ½, from whence there proceeds a different compoſition of motion from that which happens when the earth is in the tropics. According to this author, there reſults from the difference of theſe two compound motions ſome inequalities which are the cauſe of that which is obſerved in the ſolſtices and equinoxes.

From hence he concludes, that the periods of the tide which are regulated according to the days, months, and years, all have for their firſt and principle cauſe the annual and daily motion of the earth; and that the ſun and moon come in only by accident.

Deſcartes, who ſeems better informed than *Galileo* in the *phænomena* of the tides upon the coaſts of the ocean, and who knew that they there obſerved regularly the flux and reflux of the ſea twice a day, in the ſpace of 24 hours, 48 minutes, or thereabouts, ſo that the ſea takes exactly 6 hours 12 minutes in riſing, and as much in falling, attributed the principal cauſes of this phœnomenon to the motion of the moon.

He judged that the celeſtial matter, which ſurrounds the earth being moved by the daily motion with more velocity than the earth, was incloſed between the earth and moon, which obliged the earth to give way a little to the oppoſite ſide. That by this means its waters were compreſſed on both ſides, according to the direction of the moon to the earth, which made them flow back on both ſides to the diſtance of 90 degrees, where the greateſt height of the ſea was. That the moon being arrived 6 hours, 12 minutes afterwards, at the diſtance of 90 degrees from the place where it was before the waters, which had been then raiſed, were compreſſed by the interpoſition of the moon, and the ſea conſequently was lower there than in any other place. And therefore it muſt have in the ſame place a viciſſitude of high and low ſea in the ſpace of 12 hours, 24 minutes, as has been obſerved there.

As to the tides which in each lunation are higheſt in the new and full moons, and loweſt in the quadratures, he pretends that this proceeds from the direction of the vortex of the earth, which according to his ſyſtem, is elliptical, and has its leſſer axis directed toward the ſun. That in the conjunctions and oppoſitions the moon is in this direction, and conſequently the flux and reflux of the ſea muſt be greater than in the quadratures, when the moon is ſituated in the direction of the greateſt axis of the ellipſis.

He adds at laſt, that the moon being always in a plane near the ecliptic, and the earth making its daily revolution according to the plane of the equator, both theſe planes interſect one another in the equinoxes, whereas in the ſolſtices they are very diſtant from one another; from whence it follows, that the greateſt tides muſt happen toward the beginning of the ſpring and autumn.

Kepler in his lunar aſtronomy * attributes the cauſe of the flux and reflux of the ſea to the bodies of the ſun and moon, which attract the waters of the ſea by a virtue almoſt like that of the loadſtone. He owns it is difficult to explain by this means why the flux of the ſea is as great at midnight when the ſun and moon are abſent, as at noon when they are preſent. However, he imagines that the flux of the night may be produced by the reflection made againſt the coaſt of *America* by the waters, that the moon has carried away with her, and reciprocally by the reflection made againſt the coaſts of *Europe* and *Africa*, by the water that the moon brought at her return.

Sir *Iſaac Newton*, in his *Principia Mathematica* adopts the opinion of *Kepler*, in attributing the cauſe of the tides to the attraction produced by the ſun and moon. According to his principles he finds, that the ſea muſt riſe twice every day, both ſolar and lunar; and that the greateſt height of the tide muſt happen in leſs than ſix hours after the paſſage of the ſun and moon through the meridian, as is obſerved in the eaſtern parts of the

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* Page 70.

Atlantic and *Ethiopic* oceans, between *France* and the *Cape* of *Good-Hope*, and upon the coaſts of *Chili* and *Peru* of the *Pacific* ocean where the flux of the ſea happens much about the third hour.

Theſe two motions which the ſun and the moon produce, are not perceived diſtinctly, but make a compound motion. In the conjunctions and oppoſitions theſe two effects are joined together, and form the greateſt flux and reflux. In the quadratures the ſun raiſes the water in the place where the moon depreſſes it, and the fluxes and refluxes of the ſea, which reſult from this difference, are the leaſt that can happen in the courſe of a month, and becauſe according to experience the effect of the moon is greater than that of the ſun, the greateſt height of the ſea muſt happen at the third lunar hour. He calls the lunar hour the 24th part of the time that the moon takes up in returning to the meridian of the ſame place.

Sir *Iſaac Newton* alſo judges that the effects of the ſun and moon are greater in their leaſt diſtances from the earth than in the greateſt, and that in a triple ratio of the apparent diameters. That conſequently, all things being equal, the ſun being in the winter in its *perigeum* the tides muſt be a little greater than in ſummer; and that the moon being in its *perigeum*, the tides muſt be greater than 15 days before or after, when it is in its *apogeum*.

He adds, that the effect of the ſun and moon depends upon its declination or diſtance from the equator. That if both theſe planets were in the direction of the pole, they would attract all the waters uniformly; ſo that there would not be any reciprocal motion; and therefore the ſun and moon in receding from the equator towards the poles, loſe by degrees their effort, and make the tides ſmaller in the ſyzygies of the ſolſtices, than in thoſe of the equinoxes; but in the quadratures of the ſolſtices, the tides muſt be greater than in the quadratures of the equinoxes, becauſe the effect of the moon which is then in the equator exceeds that of the ſun.

The greateſt tides therefore happen in the ſyzygies, and the ſmall in the quadratures which are about the time of the equinoxes, and the greateſt tide of a ſyzygy is followed by the leaſt of a quadrature, which he ſays agrees with experience. It alſo proceeds from the diſtance of the ſun from the earth, which is leſs in winter than in the ſummer, that the greateſt and ſmalleſt tides oftener precede the vernal equinox than follow it; and oftener follow the autumnal equinox than precede it.

Sir *Iſaac Newton* finds afterwards, that the effects of the ſun and moon depend alſo upon the latitude of places; that we may conſider the ſea as divided by the flux of the ſea into two hemiſpheroides, one of which is toward the north, and the other toward the ſouth; that the tides of both theſe oppoſite hemiſpheroides paſs ſucceſſively through the meridian of each place in 12 hours; that the northern countries partake more of the north tides, and the ſouthern of the ſouth tides; and therefore, out of the equator the tides of each day are alternatively greater and ſmaller. The greateſt tides happen 3 hours after the moon's paſſage through the meridian, when this planet declines from the equinoctial towards the zenith, and the moon changing its declination the tides will be ſmaller.

The greateſt difference between the tides of the ſame day muſt happen at the time of the ſolſtices, eſpecially, when the aſcending node of the moon is at the beginning of *Aries*. It is alſo found by experience, that in the winter the morning tides are greater than the evening ones; and in the ſummer, the evening tides are greater than the morning ones, at *Plymouth*, by about a foot, and at *Briſtol* 15 inches.

The other opinions of philoſophers concerning the cauſe and effects of the flux and reflux of the ſea,are almoſt all reduced to thoſe which I have juſt related; we have therefore thought it neceſſary to examine what thoſe are which agree with experience, and which are thoſe that are contrary thereto.

As to what *Poſſidonius* relates, heh as very well diſtinguiſhed the three motions of the tides which follow the periods of the days, months, and years; but he ſuppoſes that the tides are greater towards the ſolſtices than toward the equinoxes, which is not conformable to experience. Perhaps they had obſerved the tides in the ſummer ſolſtice when the moon was very near the earth, in which caſe the ſea muſt have riſen to a great height, which had given room to conjecture that the greateſt tides always happen in the ſolſtices, and the ſmalleſt in the equinoxes. In ſhort, *Strabo*, who relates the opinion of *Poſſidonius*, ſays, that this author having been at *Cadiz*, in the temple of *Hercules*, about the ſummer ſolſtice, had not obſerved any extraordinary tides in the full moon, but that about the new moon in the river *Bætis*, or *Quadalquiver*, there happened ſo great an overflowing of the waters, that the level of the ground of the light-houſe, and the rampart of the port of *Cadiz* had been covered to the height of 10 cubits.

Pliny's opinion concerning the tides of the equinoxes and ſolſtices, ſeems more agreeable to the obſervations, as he affirms, that the greateſt tides happen in the equinoxes, and the ſmalleſt in the ſolſtices; but he advances that the tides are yet greater in the autumnal equinox than in the vernal, which we have not been able to diſcover by the obſervations. What is moſt remarkable is, that he has found that the tides are ſmaller when the moon receeds from the earth, than when it approaches it; and its force acts nearer, which agrees with our obſervations. He alſo obſerves, that in the ſpace of eight years, after 100 revolutions of the moon, there are the ſame principles of motion of the tides, and the ſame augmentations obſerved, which has a great deal of relation to the motion of the moon's *apogeum*, which in eight or 9 years returns to the ſame point of the Zodiack after 118 revolutions of the moon, which we may conſequently take for the period of the principal inequalities that are obſerved in the tides. He adds, laſtly, that the effects produced by the motions of the celeſtial bodies are not ſo ſoon felt upon the earth, as they are perceived by the eye, which agrees perfectly with our obſervations.

With regard to *Galileo*, who pretends that the principal cauſe of the *phænomena* obſerved in the flux and reflux of the ſea, muſt be attributed to the motion of the earth, it would be difficult to reconcile his opinion with the obſervations. He allows himſelf, that according to his principles, there muſt be only one flux and reflux in 24 hours, and that if it ſometimes happens oftener, it proceeds from ſeveral accidents, as the depth of the water, the directions of the coaſts of the ſea, *&c.* But if it was ſo, how ſhould we be perſuaded that theſe accidental cauſes, which vary in ſo many different manners, according to the different places, ſhould cauſe ſo regular an effect, that inſtead of one flux and reflux in 24 hours, there ſhould be two fluxes and refluxes in 24 hours, 48 minutes? An effect which is known to every body, and which we have obſerved in all the ports of *France*, that are upon the ocean, where, notwithſtanding the various depth of the water, and the different directions of the coaſts, the tide regularly takes up 6 hours and about $\frac{1}{4}$ in riſing, and as much in falling.

Beſides the periods of the flux and reflux of the ſea, which we obſerve every day, the cauſe of which *Galileo* attributes to the motion of the earth, he finds, that according to his ſyſtem, there ought to be in the tides a period regulated according to the revolution of the moon with regard to the ſun; the earth having, as he conjectures, a greater degree of velocity in the conjunctions than in the oppoſitions. To this may be anſwered, that theſe different degrees of velocity, which he attributes to the earth in the new and full moons, were not known till now by the aſtronomers. But tho' this effect, which perhaps is not ſenſible enough to be obſerved by aſtronomical obſervations, ſhould be enough to make an impreſſion upon the ſea as *Galileo* ſuppoſes, it would follow from thence that the tides which happen in the conjunctions would be different from thoſe obſerved in the oppoſitions, and that thoſe of the quadratures would be the moſt uniform; which does not agree with the ob-

obſervations by which it has been found, that the greateſt tides happen equally in the conjunctions and oppoſitions where they are pretty uniform; and that the ſmalleſt are obſerved in the quadradratures, where they are ſubject to more irregularities. Nor does the reaſon which *Galileo* relates for the annual periods of the tides ſeem to agree with the obſervations; for in the ſolſtices, the daily motion of the earth being made in the ſame direction as the annual one, it would ſeem that the compoſition of theſe two motions ſhould cauſe greater tides than in the equinoxes, where the directions of both theſe motions are inclined to one another; and yet on the contrary we obſerve, that the tides are greater in the eqninoxes than in the ſolſtices.

The different degrees of velocity of the annual motion of the earth when it is in its *Perihelion*, or *Aphelion*, muſt alſo cauſe according to this opinion, a very great alteration in the tides; nevertheleſs we do not obſerve any conſiderable variation in the tides of the winter ſolſtice, when the earth moves with more velocity than in the ſummer ſolſtice, when it moves ſlower.

Deſcartes's opinion concerning the cauſe of the flux and reflux of the ſea ſeems more agreeable to our obſervations; for it is eaſy to imagine, that all the celeſtial bodies making by their motion ſome impreſſion upon one another, the earth is obliged to give way on the oppoſite ſide to the moon; ſo that the waters of the ſea are compreſſed according to the direction of the moon to the earth, and forced to flow back on both ſides to the diſtance of 90 degrees, which makes the high tide.

The reaſon which he gives why the tides are greater in the ſyzygies than in the quadratures, is a conſequence of his ſyſtem, in which he ſuppoſes, that the leſſer axis of the vortex of the earth, which is elliptical, is always pointed toward the ſun; ſo that the moon is nearer the earth in the ſyzygies than in the quadratures. But this does not always agree with the aſtronomical obſervations; for it is true, that the moon being in the ſyzygies, and at the ſame time in its *perigeum*, is nearer the earth than in any other phaſe; but we cannot conclude from hence, that the leſſer axis of the vortex of the earth which carries the moon, is always directed toward the ſun; for it often happens that the moon is nearer the earth in the quadratures than in the ſyzygies; and yet we always obſerve, that in the quadratures the tides are leſs than in the ſyzygies.

We cannot therefore attribute the cauſe of the great tides in the new and full moons to the proximity of the moon to the earth, and that of the ſmall tides in the quadratures to its diſtance; and this has given us room to conjecture that the ſun as well as the moon concurs in producing the height of the tides, altho' its effect was not ſo conſiderable as that of the moon; that in the ſyzygies both theſe cauſes acting according to the ſame direction, the tides muſt be greater than toward the quadratures when the ſun acted in a direction perpendicular to that of the moon.

Kepler, and after him Sir *Iſaac Newton*, have judged, that the ſun as well as the moon contributes to the height of the tides, with this difference, that as we ſuppoſe the tides are produced by the preſſure of the ſun and moon upon the ceſtial leſtial matter which ſurrounds the earth, they have attributed this effect to the bodies of the ſun and moon, which attract the waters of the ſea by a vertue almoſt like that of the loadſtone. Theſe two hypotheſes, tho' very different in their principles, ſeem to be equally able to give a reaſon for all the *phænomena* which are obſerved in the tides. It is true, that according to the ſyſtem of the preſſure, the ſea muſt be low in the places where it ſhould be high according to the ſyſtem of the attraction; but as in the new and full moons the high-water happens in ſeveral places at different hours of the day, before and after noon, it is not eaſy to determine to which of theſe cauſes we ought to attribute the flux and reflux of the ſea. It is therefore more proper before we embrace any ſyſtem to be aſſured of a great number of obſervations, which we have hitherto done, and have had an opportunity of continuing by a new journal of obſervations upon the tides made at *Breſt* during the years 1712, and 1713.

This journal begins the 13th of *July*, 1712, where the preceding had finiſhed, and has been continued to *March* 1713.

In this interval, which is about nine months, they have obſerved the tides of 18 both new and full moons, among which are found thoſe of the autumnal equinox, the winter ſolſtice, and the vernal equinox.

The tide which happened the ſooneſt was obſerved *Feb.* 24, 1713, at 3 hours, 6 minutes, in the morning, the new moon being marked that day in the *Connoiſſance des Temps*, at 10 hours, 50 minutes, at night. That which happened the lateſt was obſerved *Dec.* 13, at 4 hours, 27 minutes, ½, the full moon being marked that day at

1 hour, 3 minutes, in the morning. The difference between theſe two obſervations, which is 1 hour, 24 minutes ½, may be partly corrected, by ſuppoſing as in the preceding memoirs, the mean time of the full moon at *Breſt*, at 3 hours, 45 minutes, and making uſe of the common equation of 2 minutes for an hour, which muſt be added to the mean time, or ſubtract from it, according as it retards or anticipates with regard to the time of the new or full moon. For we ſhall find, that the 24th of *Feb.* 1713, the day of the new moon, and of the greateſt acceleration of the tide, the high tide ought to have happened at 3 hours, 7 minutes, within a minute near what it was obſerved at; and that the 13th of *Dec.* the day of the full moon, and of the greateſt retardation, the height of the tide ought to have happened at 1 hour, 14 minutes ½, within near 12 minutes ½, of what was obſerved.

The obſervation of the 24th of *February* is marked for the greateſt acceleration in the ſpace of almoſt two years, and is diſtant from the mean time of the high tide 39 minutes, which by the equation preſcribed, are reduced to one minute; and the obſervation of *December* 13, is that where the greateſt retardation was found in the ſame ſpace of time, and is 42 minutes diſtant from the mean time, which by our equation are reduced to 12′ ½, which ſhews the neceſſity of uſing this equation, and the advantage that may be drawn from it, for knowing more certainly the time of high-water, the days of the new and full moon.

As to the time of high-water in the quadratures, it is ſubject to great inequalities, which however will be partly corrected, by ſuppoſing the mean time of the high tide at *Breſt*, on the day of the quadratures, at 8^h 57′ as has been done already, and uſing the common equation of 2′ ½ in an hour inſtead of that of 2′, which we ſuppoſe in the new and full moons.

They have been obliged in the quadratures to make uſe of this horary equation of two minutes and a half, becauſe they have obſerved, that one day with another, the tides retard much more toward the quadratures, than toward the new and full moons ; and for this reaſon

In the new and full moons, the preſſure is greater than toward the quadratures, and conſequently the effect, produced upon the waters of the ocean which are in the high water, take up leſs time to communicate themſelves towards the coaſts, than in the quadratures where the preſſure of the moon being much leſs, its effort employs much more time to communicate itſelf toward the coaſts, and cauſes a retardation in the tides, which makes an effect like the waves of the ſea, which are greater, and acquire a greater velocity, the greater the force is which acts them.

We have already obſerved, that the ſea takes up more time in falling than in riſing. This is confirmed by theſe new obſervations ; and it ſeems, that we might attribute the cauſe of it to this ; that the effort which obliges the waters to riſe, drives them with violence, and conſequently with a great deal of velocity toward the coaſts, from whence they afterwards retire by their own weight with leſs velocity than they were raiſed.

With regard to the tides which happen twice in the ſame day, every twelve hours, they muſt continually change in height, ſince in each month they are greateſt one or two days after the new and full moons ; that they afterwards diminiſh continually till a day or two after the quadratures; that they afterwards riſe again, and ſo on ſucceſſively; but beſides this inequality of height, which they have obſerved at all times, it alſo meets with others. For we often obſerve, that from the quadratures to the new and full moons, the evening tide which ought to be greater than that of the morning, becauſe the tides increaſe from day to day, is however ſometimes ſeveral inches leſs in the morning than at night; and on the contrary, in other circumſtances, from the new and full moons to the quadratures, they find the evening tide greater than that of the morning, tho' it ought to be leſs, becauſe the tides from one day to another continually diminiſh.

This inequality of height in the tides has been, by Sir *Iſaac Newton*'s account, obſerved at *Plymouth*, and at *Briſtol*, by Meſſ. *Colepreſs* and *Sturmius*, who have obſerved that in the winter, the morning tide was greater than the evening one; and that in the ſummer, it was leſs than the evening one.

Perhaps they made theſe obſervations about the new and full moons which happen near the ſolſtices, when there is in effect, almoſt always this variety of height in the tides which ſucceed one another every twelve hours.

For example, the 19th of *June*, 1712, the day of the full moon, the morning tide was obſerved at *Breſt* 17 feet 1 inch, leſs by a foot and an inch than that of the evening, which was found to be 18 feet 2 inches.

The 2d of *July*, the day after the new moon, the morning tide was obſerved 14 feet 10 inches 5 lines, leſs by 9 inches 2 lines than that of the evening, which was found to be 15 feet, 7 inches 8 lines.

On the contrary, the 13th of *December* 1712, the day of the full moon, the morning tide was obſerved to be 16 feet 7 inches, greater by 4 inches 4 lines, than that of the evening, which was found to be 16 feet 2 inches and 8 lines.

The 29th of the ſame month, the day of the new moon, the morning tide was obſerved to be 18 feet 10 inches, greater by 7 inches than that of the evening, which was 18 feet 3 inches.

It appears therefore, by theſe obſervations, as by many others, which would be too long to relate, that about the new and full moons, the ſummer tides are leſs in the morning than at night; and the winter tides are leſs at night than in the morning, which may eaſily be accounted for, if we ſuppoſe, conformable to our hypotheſes, that the tides are almoſt of equal height in the parts of the earth directly oppoſite to one another.

In ſummer, in the new moon, this planet paſſes about noon with the ſun through our meridian, with a northern declination, and conſequently its greateſt force muſt be felt in the northern countries of our hemiſphere, and in the ſouthern countries of the other hemiſphere, which are directly oppoſite to us. About twelve hours after, toward midnight, the moon paſſes throngh the meridian in the oppoſite hemiſphere, with a like declination; and conſequently its greateſt force muſt be perceived in the northern countries of the other hemiſphere, and in the ſouthern countries of our hemiſphere, which are directly oppoſite to it: the effort or preſſure of the moon in the ſummer new moons, is therefore greater at noon in the northern countries, where we live, than in the ſouthern countries; and on the contrary, this preſſure is greater at midnight in the ſouthern than in the northern countries; from whence it follows, that the height of the tide being proportioned to the different efforts or preſſures of the moon, the evening tide immediately after noon muſt be greater in the new moons of the ſummer, than the tide which happens after midnight.

In the full moons which happen alſo in the ſame ſeaſon, the moon paſſes over our meridian at midnight, with a ſouthern declination, and conſequently the morning tide, which immediately follows, muſt be leſs than that of the afternoon, the moon paſſing at noon over the meridian of a place which has the northern part of the earth for its oppoſite, where the greateſt evening tide muſt happen.

On the contrary, in the winter new moons, this planet paſſes with the ſun through the meridian with a ſouthern declination, and conſequently the preſſure which it occaſions in the northern countries muſt not be then ſo great as thoſe which it produces in the ſouthern countries, and the evening tide, which immediately follows, muſt be leſs than that of the morning, the moon paſſing at midnight through the meridian of a place, which has its oppoſite in the northern part of the earth, where the greateſt morning tide muſt happen. In the winter full moons, the moon paſſes at midnight through the meridian with a northern declination, and conſequently the morning tide which follows immediately, muſt be greater than that of the evening, the moon paſſing at noon through the meridian of a place which has its oppoſite in the ſouthern part of the earth, where the greateſt evening tide muſt happen.

It appears therefore by this reaſoning, that in the new and full moons, the ſummer tides muſt be leſs in the morning than at night; and that the greateſt tide muſt happen at night. That on the contrary, in the winter, the tides are leſs at night than in the morning; and that the greateſt tide muſt happen in the morning.

We obſerve only that the difference of height between the morning tide, and evening tide, is greater in ſummer than in winter; which muſt in effect happen, becauſe the evening tides, which in ſummer are greater than thoſe of the morning, by the reaſons juſt related, are alſo increaſed in height by the augmentation which is made in the *tides* every twelve hours, from one or two days after the quadratures, to one or two days after the new or full moon; whereas in winter, the evening tide, which is leſs than the morning one, is increaſed in height, becauſe the tides which increaſe in the new and full moons, are greater at night than in the morning; which cauſes a leſs difference in the height of the tides of the ſame day in winter than in ſummer.

There are therefore two cauſes, which concur to the variation of height which is obſerved in the tide, which happen in one day every twelve hours; one, which is produced by the continual augmentation or diminution which happen between the new and full moons, and the quadratures; and the other, which we muſt attribute to the different height of the moon upon the horizon; according to which its declination is more to the north or ſouth. This laſt cauſe commonly prevails above the other; when the moon is in the northern or ſouthern ſigns, but it is hardly ſenſible in the new or full moons of the equinox, when the moon paſſes through the meridian with very little declination.

This augmentation or diminution in the tides every twelve hours, as we have juſt obſerved in the new and full moons which happen about the ſolſtices, muſt be obſerved alſo in the quadratures which happen about the equinoxes.

In the vernal equinox, the moon is in its firſt quarter in the northern ſigns, and paſſes through the meridian at ſix at night, with a northern declination, and conſequently the evening tide, which follows its paſſage through the meridian, muſt be greater than that of the morning. In the third quarter, the moon paſſes through at 6 in the morning, with a ſouthern declination, and conſequently the morning tide muſt be leſs than the evening one. Thus in the quadratures, which happen about the vernal equinox, the tides are leſs in the morning than at night, and the leaſt muſt happen in the morning.

On the contrary, about the autumnal equinox, the moon in its firſt quarter is in the ſouthern ſigns, and paſſes through the meridian at ſix at night, with a ſouthern declination, and conſequently the evening tide, which follows its paſſage through the meridian, muſt be leſs than that of the morning. In the third quarter, the moon paſſes through the meridian at 6 in the morning, with a northern declination, and conſequently the morning tide, which follows its paſſage through the meridian, muſt be greater than that of the evening.

Thus in the quadratures which happen about the autumnal equinox, the morning tides are greater than thoſe of the evening, and the leaſt tides muſt happen in the evening.

This reaſoning agrees pretty well with the obſervations for the 6th of *September* 1711;

the day of the leaſt tide of the autumnal equinox, the evening tide was obſerved to be 10 feet 3 inches, leſs by 7 inches than the morning tide which was 10 feet 10 inches. The 23d of *September* 1712, the day of the leaſt tide of the autumnal equinox, the evening tide was alſo obſerved to be ten feet eight inches four lines, leſs by nine inches eight lines, than the morning tide which was eleven feet 6 inches.

On the contrary, the 16th of *March* 1712, the day of the leaſt tide of the vernal equinox, the morning tide was obſerved to be 10 feet 10 inches, leſs by 3 inches than that of the evening; and the 20th of *March* 1713, the day of the leaſt tide of the laſt quarter, the morning tide was obſerved at 11 feet 7 inches, leſs by nine inches than that of the evening.

We have obſerved in the preceding memoirs, that the various diſtances of the moon from the earth cauſe a very great variety in the height of the tides. This is confirmed by theſe laſt obſervations, for the 28th of *Dec.* 1712, the day of the full moon, the diſtance of this planet from the earth, being 936 parts, the radius of which is 1,000; that is to ſay, the moon being very near its *perigeum*, they obſerved, *Dec.* the 30th, in the morning, the day of the greateſt tide, the height of the full ſea to be 19 feet, 2 inches, above the fixed point, and that of the low-water to be 1 foot, 8 inches, below this point, ſo that the ſea had riſen that day to the height of 20 feet, 10 inches.

The 11th of *Jan.* following, the day of the full moon, this planet's diſtance from the earth being

1064, that is the moon being very near its *apogeum*, they obſerved the 15th of *Jan.* in the morning, the day of the greateſt tide, the height of the full ſea to be 17 feet, 5 inches; and that of the next low-water to be 1 foot; ſo that the ſea had only riſen 16 feet, 5 inches, that day, leſs by 4 feet, 5 inches, than in the preceding obſervation when the moon was near its *perigeum*.

It muſt be obſerved, that in the new moon *perigeum* of the 28th of *Dec.* 1712, its declination was 23 degrees to the ſouth, very diſtant from the equinoctial, and conſequently its preſſure upon the earth muſt not be ſo great as when the moon being almoſt at equal diſtance from the earth, is at the ſame time nearer the equator.

In effect we find, that the 24th of *Feb.* 1713, the day of the new moon, its diſtance from the earth being 953, that is, near its *perigeum*, and its declination 5 degrees ſouth near the equator, the height of the full ſea was obſerved *Feb.* 26, in the morning to be 21 feet, 2 inches, which is the greateſt height that they have obſerved at *Breſt* in almoſt two years. The low-water following was obſerved to be 1 foot, 3 inches, below the fixed point, ſo that the tide roſe that day to the height of 22 feet, 5 inches.

The 12th of *March* following, the day of the full moon, its diſtance from the earth being 1,032, pretty near its *apogeum*, and its ſouthern declination one degree, that is, near the equator. They obſerved the 13th of *March* following, the day of the greateſt tide, the height of the full ſea to be 18 feet, 2 inches, and that of the low-water 0 feet, 0 inches; ſo that the elevation of the ſea that day was only 18 feet, 2 inches, leſs by 4 feet, 3 inches, than in the preceding obſervation,

vation, when the moon was near its *perigeum*, but 1 foot, 9 inches, greater than in the obſervation of *Jan.* 11, 1713, before related, when the moon being near its *apogeum*, its northern declination was 20°.

As to the ſmall tides which follow the quadratures, we find alſo by theſe laſt obſervations that their heights are proportioned to the different diſtances of the moon from the earth. For example, *Sept.* 23, 1712, the day of the leaſt tide which followed the 3d quarter, the moon's diſtance from the earth being 1,063 very near its *apogeum*, the height of the full ſea at night was obſerved to be 10 feet, 8 inches, 8 lines, and that of low-water 5 feet, 10 inches, ſo that the ſea roſe that day only 4 feet, 10 inches, 8 lines. The 7th of *Oct.* following, the day of the leaſt tide which followed the 1ſt quarter, the moon's diſtance from the earth being 976 near its *perigeum*, the height of the full ſea was obſerved at night to be 12 feet, 10 inches, and that of the morning 3 feet, 6 inches, ſo that the elevation of the tide was that day 9 feet, 4 inches, greater by 4 feet, 5 inches, 4 lines, than in the preceding obſervation, when the moon was near its *apogeum*.

The moon's northern declination was *Sept.* 22, 1712, the day of the laſt quarter, 24° ½, and conſequently the ſmalleſt tide following muſt be very low as was obſerved, having been found the 23d at night to be 10 feet, 8 inches, 8 lines, lower by 2 feet, than the 26th of *Dec.* when the diſtance of the moon from the earth being 1,036 near the *apogeum*, and its ſouthern declination 5 degrees, the height of the tide was found to be 12 feet, 8 inches, 8 lines.

Beſides the variations in the height of the tides which reſult from the different diſtances of the moon from the earth, and its different declination with regard to the equinoctial, there muſt alſo according to our hypotheſis be ſome that are cauſed by the different diſtance of the ſun from the earth, and by its different declination, we have already obſerved that the tides of the new and full moons were greater than toward the equinoxes, when the ſun has no declination, than toward the ſolſtices, when it has 23 degrees, 29 minutes, and it is probable that the ſun which is then in conjunction, and in oppoſition with the moon, concurs with it to the different heights which are obſerved.

With regard to the diſtance of the ſun from the earth, as it is leſs about the winter ſolſtice when the ſun is juſt at its perigeum, than at the ſummer ſolſtice when it is near its *apogeum*, the tides muſt be greater in winter than in ſummer, all things being equal, as in effect it is obſerved. For *July* 30, 1711, the day of the full moon, its diſtance from the earth being 960, and its declination 25 degrees, 29 minutes, the ſun being alſo in its *apogeum*, they obſerved *July* 1ſt at night the height of the greateſt tide to be 17 feet 10 inches. The 8th of *January* following, the day of the new moon the moon's diſtance from the earth being 951, and its declination 23 degrees, almoſt the ſame as the 30th of *July*, the ſun being then near its *perigeum*, they obſerved *January* 10th in the morning, the height of the greateſt tide to be 19 feet 10 inches, higher by 2 feet than in the preceding obſervation, when the ſun was in its *apogeum*. The 19th of *June* following, the moon's diſtance from the earth being 936, and its ſouthern declination 24 degrees 50 minutes, the ſun being then near its *apogeum*, the height of the greateſt tide was obſerved *June* 21 at night, to be 18 feet 4 inches, leſs by 1 foot 6 inches than in the preceding obſervation. Laſtly, the 28th of *December* 1712, the ſun being in its *perigeum*, the moon's diſtance from the earth being 936, and its ſouthern declination 23 degrees, the height of the greateſt tide was obſerved the 30th of *December* to be 19 feet 2 inches, greater by 10 inches than the 19th of *June*, when the ſun was near its *apogeum*, and the moon at almoſt equal diſtance from the earth.

It reſults therefore from theſe obſervations, that there are four cauſes which contribute to the different heights that are obſerved in the tides. The firſt depends upon the different ſituations of the moon, with regard to the ſun, and produces the variations that are obſerved in the height of the tides, from the new and full moons to the quadratures. The ſecond is produced by the different diſtances of the moon from the earth, the tides being greater, when the moon is near its *perigeum*, than when it is near its *apogeum*. The third is produced alſo by the different diſtances of the ſun from the earth, the tides being greater when the ſun is in its *apogeum*, than when it is in its *perigeum*. Laſtly, the fourth depends on the diſtance of the moon from the equinoctial, the tide being leſs when the moon has a great declination than when it is near the equator. This laſt cauſe produces alſo the variations that are obſerved in the tides which happen in the ſame day. Theſe variations muſt be perceived differently in different parts of the earth; they muſt be null in the countries that are under the equinoctial line; but they are very ſenſible in the northern and ſouthern countries, according as the declination of the moon is more to the north or ſouth.

VII. *A deſcription of a portable machine, proper to ſupport the glaſſes of very great foci; preſented to the academy by M.* Bianchini, *by M.* de Reaumur*.

The number of obſervations, with which M. *Bianchini* has enriched our memoirs, have ſufficiently informed the publick of his ability and attention in obſerving the heavens; but his zeal for aſtronomy does not end there, for knowing better than any body how far this ſcience has been carried ſince they have known how to work the great glaſſes, he has thought of rendering the uſe of them eaſy. Meſſ. *Huygens* and *Caſſini* did a great deal in ſhewing us that we might uſe glaſſes with greater *foci* without tubes; this was removing a conſiderable difficulty by informing us that we might do without inſtruments, which it was almoſt impoſſible to conſtruct, and which it was not eaſy to make uſe of. Notwithſtanding this fine diſcovery there ſtill remained many difficulties: for placing theſe glaſſes, there muſt be wooden towers erected ſuch as there is in the obſervatory; or ſeveral ſolid beams raiſed upon the ground: it muſt have large ſpaces of ground; and beſides all theſe inconveniences, it was neceſſary to employ ſeveral perſons and different machines to change the direction of the object-glaſs, according as the ſtar ſhould change its place; the expences which this required were above the fortune and zeal of many private perſons.

This made M. *Bianchini* think that nothing would be more proper to multiply the number of obſervations, or which is the ſame thing, to perfect aſtronomy, than a machine which had the following qualities: 1ſt, that it ſhould ſuſtain the object glaſs very high, altho' light. 2d, that the height might be eaſily altered according to the different elevations of the planets above the horizon. 3d, that it ſhould be ſolid and firm, without its being neceſſary to make uſe of nails to fix it, or ſinking beams into the earth. From the preceding qualities it was alſo eſſential that there ſhould reſult two others; that the whole machine might be eaſily carried about, and coſt but little.

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* July 29, 1713.

He ſought for this machine, which did not appear eaſy to be found, and deſired M. *Chiarelli*, a prieſt of *Vincentia*, famous in *Italy* for optical works, to ſeek for it in concert with him; *Galileo*, who has done ſo much for the ſciences, was alſo of ſervice to them on this occaſion. M. *Bianchini* knowing what this celebrated author has demonſtrated upon the force of a hollow cylinder, did not doubt but that he might contrive a ſupport for the object glaſs high, ſolid, and at the ſame time light, by making uſe of hollow cylinders, that is, by cauſing tubes of different diameters to be made which ſhould ſhut into one another like thoſe of the teleſcopes; the uſe of teleſcopes ſo familiar to M. *Bianchani* had conducted him ſo far. He had already a ſupport high, light, and of which the height might eaſily be varied; two of the qualities eſſential to the machine ſought for: there only remained to find a ſolid and convenient manner of raiſing this high ſupport perpendicular to the ſurface of the earth. This is what M. *Chiarelli* has very ingeniouſly executed, and has given the machine all the advantage we could wiſh, as will be ſeen by the deſcription that we are going to give of it.

A hexagonal tube * AB about four feet and a half high, ſerves ſometimes for a caſe and ſometimes for a baſe or ſupport to ſix other tubes: The breadth of each of the faces of this great tube is pretty near two inches and ½; it is compoſed of ſix ſmall planks either glued or nailed together; the perfection of the machine conſiſts in theſe planks being thin and of a light wood.

The ſecond tube BC is only different from the firſt in its ſize; it muſt enter conveniently into the other but not wave in it, it is longer than that which receives it, tho' its diameter is leſs, by which means it may more eaſily be taken out, or which is ſtill better the upper end of each tube has a little rim, a ſort of little collar, which cannot enter into the tube that ſerves for a caſe to it; in the ſecond tube there is incloſed a third tube CD, in the ſame manner as the ſecond is incloſed in the firſt, and ſo on.

That the tube which ſerves ſometimes for a baſe and ſometimes for a caſe to all the reſt may be ſupported perpendicularly to the horizon, there are three feet HHH, which like three buttreſſes make an acute angle with the horizon, and are ſupported againſt three of the faces of the tube. The manner in which the feet ſuſtain it is very ingenious; a little hexagonal tube I, which is only a few inches high, like a ſort of ring ſurrounds part of the great tube between its aperture and the middle; it can riſe or fall freely, but is always pretty near within the bounds that we have given it; for the more convenient explaining ourſelves we ſhall call it a ring.

To this ring are faſtened with hinges three wooden tringles HHH; theſe tringles are equal between themſelves, and of the ſame length with the

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* Plate 6, Fig. 13

the great tube or a little longer; each of them are faſtened over againſt a different face; their breadth is alſo almoſt equal to that of one of the faces of the ſame tube. Theſe three tringles are the three feet of the machine: as they are held by hinges; it is eaſily imagined that by placing the ring, to which they are joined, between the middle and the upper end of the great tube, and giving it an almoſt equal inclination, they ſupport the great tube on three ſides, and hold it in a vertical poſition.

But as there might be ſome danger of one of the feet ſlipping, this has been remedied by faſtening with a hinge to the extremity of each foot a little tringle KKK of the ſame breadth with the foot, and as long as $\frac{2}{3}$ or thereabout of the great tube; this tringle is joined alſo by its other extremity with a ring L, like that to which the feet are faſtened; it is needleſs to ſay that this ſecond ring alſo ſurrounds the great tube, that it riſes and falls freely, and that it is placed near the lower end of the tube, when we would raiſe the machine perpendicularly; it is evident that in this laſt diſpoſition the three lower tringles hinder the three feet from parting.

The manner in which theſe feet and the tringles which hold them are joined together, ſhew that if we raiſe the upper and lower ring along the great tube, we at the ſame time oblige the feet and the tringles to lay upon the tube, the whole machine then takes up little room, as appears in the figure MILNN, and is ſo light that a man may conveniently carry it under one arm.

When we would make uſe of it, we begin by laying it upon the ground, or putting it in a very inclined poſition; we then draw each tube out of the caſe that holds it as far as is thought neceſſary, and keep it in the place where we would have it remain in a manner equally plain and convenient; for this purpoſe M. *Bianchini* has thought of making uſe of very thin wedges of wood, theſe wedges are eaſily taken out and as eaſily put in again; the advantage of this is that we may raiſe or lower the machine when it is prepared, according as the elevation of the ſtar requires; as it is light there is not much difficulty to put it in a vertical poſition, and to keep it in that poſition we need only unfold the feet.

It might perhaps be feared that its lightneſs would diminiſh its ſteadineſs, and this would be a well grounded fear, if we could not eaſily remedy this inconvenience. In all places where ſtones are found, it is eaſy to give the machine all neceſſary ſteadineſs; by putting ſome upon the tringles or traverſes which hold the feet, thus we load it with weights that we are not obliged to carry about.

That the great tube which ſerves for a baſe to all the reſt, may keep ſteady in the two rings that ſupport it, it is neceſſary to pierce three holes in the thickneſs of the upper ring; putting a ſcrew in each hole, and we need only ſcrew them to adjuſt the tube.

If we apprehend that the point of the ſcrews may in time pierce the wood of the tube, we may cover this tube with a band of very thin iron or copper, in the place where the ring turns when the machine is raiſed. By making a little notch quite round it, we may there fix the band, and it will not ſlip above the reſt of the tube.

It is proper alſo to make a rim at the lower extremity of this great tube, or to drive ſome nails into it, to prevent the lower ring from ſlipping off the tube when the machine is carried from one place to another ready mounted.

We ſhall not ſtay to explain at length how the object glaſs is placed at the top of this machine, it may be done different ways; but that of M. *Bianchini* is convenient. This object glaſs is generally lodged in a cylindrical tube O, which is not above 7 or 8 inches long; but this tube has a ſort of tail P above a foot long. This tail is only a wooden gutter; a ſort of tenon Q of thin wood is glued againſt the outer ſurface of the tube, toward the middle of the length of this tube there is a hole in this tenon; another tenon of wood like the former is ſet at 7 or 8 inches diſtance, and is glued under the tail P of the tube.

In the laſt of the hexagonal tubes of the machine, there is put a piece of wood in which is cut a vertical notch; this noth receives the firſt of the two tenons that are faſtened to the tube of the object glaſs; this tenon is held by the means of a ſcrew.

To the ſecond of the pieces that is fixed to the tube, or rather to the tail of the tube of the object glaſs, is faſtened a ſmall packthread. This packthread is at leaſt as long as the focus of the glaſſes; it ſerves to mark the diſtance that the eye-glaſs muſt be fixed at.

This eye-glaſs is in a tube V, like that of the object-glaſs, I mean that has a ſort of gutter which makes it a tail X, under this tail the ſecond end of the packthread Y is faſtened.

To ſuſtain the tube of the eye-glaſs, M. *Bianchini* makes uſe alſo of a ſupport Za compoſed of tubes that ſhut into one another. He has made theſe ſquare, and might have made them of any other figure, for that is very arbitrary, and there does not ſeem to be any eſſential reaſon for making the tubes that ſerve for a ſupport to the object glaſs of a hexagonal figure rather than any other.

The ſupport of the eye-glaſs is compoſed of three tubes; the laſt, that is to ſay the lowermoſt *a*, is terminated by a notch; in this notch there is put a little plank *b*, and this plank is held in the notch by a ſcrew: the thickneſs of this plank ſerves for a foot to the ſupport of the eye-glaſs: this is not a very ſolid foot, it is however a reſt, becauſe this ſupport is put in an inclined poſition, in ſuch a manner that it makes an obtuſe angle with that part of the horizon that is between it and the ſupport of the object-glaſs. The packthread, one end of which is faſtened to the tube of the object-glaſs, and the other end to the tube of the eye-glaſs, holds the ſupport of the eye-glaſs in this ſituation.

As to the reſt, it is eaſy to ſee of what height all the tubes together of this laſt ſupport ought to be: it muſt be ſuch that a man ſtanding upright, or ſetting, may apply his eye near the glaſs.

All the glaſſes with their tubes, and the ſupport of the object-glaſs, may be incloſed in a ſmall and light box; ſo that the ſame man, who under one arm carries the ſupport of the object-glaſs, may with the other, carry the box that holds all the reſt of the *apparatus:* thus an obſerver who has not near him ground enough, or ground proper for obſerving, carries when he pleaſes into the fields what is neceſſary for it, he is in a ſtate of chuſing the moſt convenient grounds for his obſervatory; he may do it alone; but it is however right to have a perſon with him to depreſs or elevate ſome of the tubes of the great ſupport, according as the ſtar aſcends or deſcends, though in caſe of neceſſity he might do the whole alone.

A high wind would not be a favourable time to obſerve with this machine; the wind however makes leſs impreſſion on it than one would imagine.

VIII. *A hiſtory of an extraordinary ſleeping, by M.* Imbert.

Sleep is the moſt melancholy and humbling ſtate of man; in health it has bounds that nature has the art of prolonging often by habit, or conſtitution. Among animals, the dormouſe, and marmottee, ſleep ſix months in the year without awaking. A ſleeper of this ſort is a rare example, the hiſtory of which has ſeemed to me to be worthy of the enquiry of a philoſopher that is a curious obſerver.

A man of about 45 years of age, of a dry and robuſt conſtitution, whoſe name was *Tally*, who drove the *Rouen* coach, and was a carpenter by trade, fell into the diſorder I am ſpeaking of by the following accident. He had quarreled with a carpenter for whom he had worked; they were parted juſt as they were going to fight, and each went his own way. A little while after, our ſick perſon heard that his adverſary had fallen from a building, and was killed. This fatal news ſeized him, he threw himſelf with his face upon the ground, and his ſpirits and ſenſes grew drowſy inſenſibly. The 26th of *April* 1713, he was carried to *la Charité*, where he remained till the 27th of *Aug.* of the ſame year, that is, 4 months. The 2 firſt months he gave no ſign of voluntary motion or ſenſation; his eyes were ſhut day and night; he often moved his eye-lids; his re-

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Fig. 1.

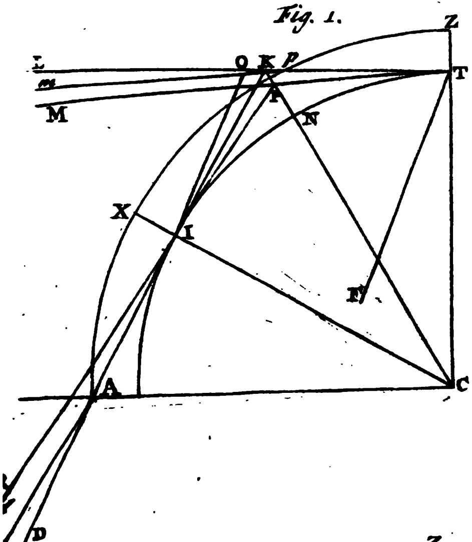
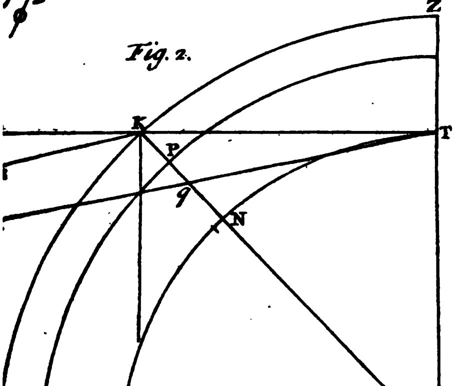
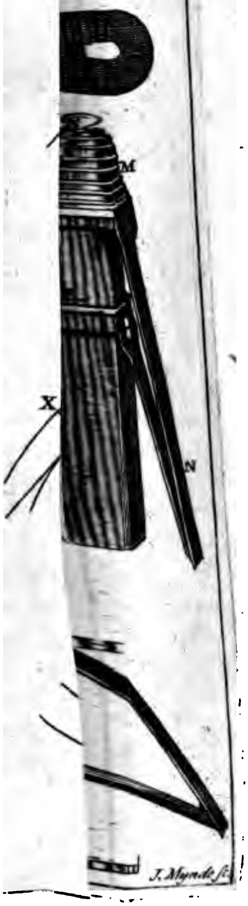


Fig. 2.





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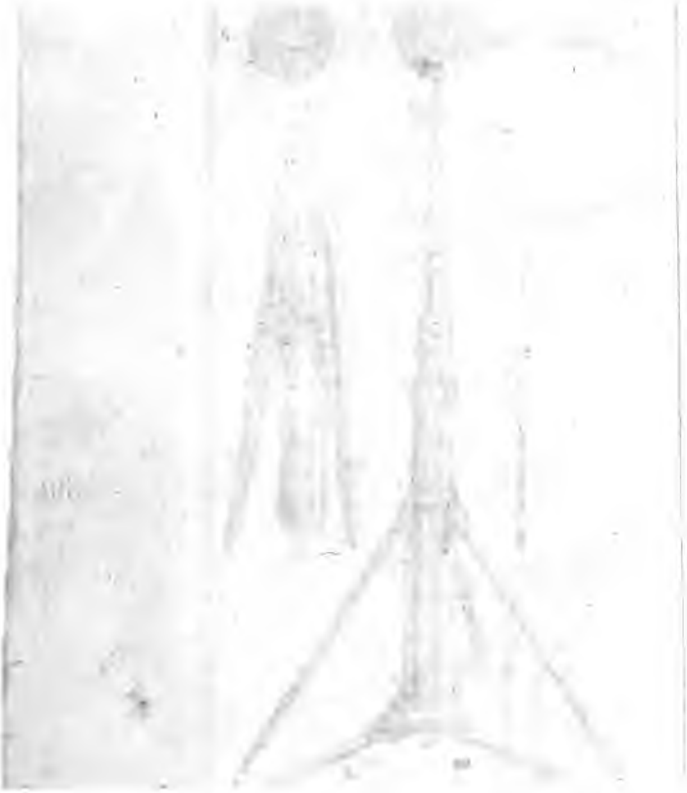

reſpiration was always free and eaſy, his pulſe was ſmall, and ſlow, but equal; if you put one of his arms in any ſituation it remained there, (a diſeaſe that is called a catalepſy); but it was not the ſame with the reſt of his body; they made him ſwallow ſome ſpoonfuls of wine to ſupport him; and this was his only nouriſhment during this time; he therefore became lean, dry, and emaciated; a very different ſtate from that which he was in before. M. *Burette*, under whoſe hands he was, at firſt made uſe of the moſt powerful aſſiſtances of art, bleeding in the arm, the foot, the neck, emetics, purgatives, bliſters, leaches, and volatiles, and this without being able to procure any other relief to him than that of talking very ſenſibly to his family, and the clergy, for an entire day, after which he fell again into his ſleeping. The two laſt months of his ſtay at *la Charité*, he by intervals gave ſome marks of ſenſation, ſometimes preſſing his wife's hand, and at other times by melancholy complainings; but this would happen when they had been ſeveral days without purging him. From this time he ceaſed to do all under him, being careful to turn himſelf to the edge of the bed, where a waxed cloth was put on purpoſe, and not to do any thing till he found himſelf there, and then he did his occaſions, and returned to his place; he began alſo to take broths, pottage, and other ſuſtinence, keeping ſtill his firſt inclinations, a great thirſt for wine. He never made any ſigns that he wanted any thing. At the times appointed for his meals, they touched his lips with their fingers; at this ſignal he opened his mouth without opening his eyes, and ſwallowed what was given him; he then lay ſtill expecting patiently a ſecond notice. They ſhaved him regularly, but he was all the time like a corps ſet upright. If he was taken up after dinner, they found him in his chair with his eyes ſhut in the ſame poſture that they had left him. A week before he went out of *la charité*, they threw him naked into cold water to ſurpriſe him. This remedy ſurpriſed him effectually; he opened his eyes, looked ſteadfaſtly, but did not ſpeak at all. In this condition his wife carried him home, where he is at preſent: they give him no medicine; he ſpeaks ſenſibly enough, and mends every day.

Here is a ſtumbling block for a philoſophical reaſoner; being always impatient to get the maſtery of nature in her moſt hidden deſigns, he ſees, admires, and ſearches, and yet diſcovers nothing. I ſhall venture however to propoſe to the company as conjectures, ſome reflections that I have made upon ſo ſingular a hiſtory. That I may repreſent them in order, I ſhall firſt examine how grief may produce this kind of ſleep; in the ſecond place I explain the different alteration which have happened to it; in the laſt place I ſeek for examples that may have ſome relation to it. In the firſt propoſition two things are to be conſidered; upon what ſleep depends, and the manner in which grief acts. There are many cauſes that produce ſleep in general: in the brain, obſtruction in the glands, compreſſion or relaxation; from hence commonly proceed apoplexies, and lethargies: in the blood, impoveriſhing of the ſpirits; and from hence proceeds the indiſpenſable neceſſity for man to ſleep to repair them; ſpirits too much incumbered by the groſs parts; and hence proceeds the diſpoſition always near to the ſleeping diſeaſes. Such was the ſtate of our pa-

patient before he fell. A carpenter by profeſſion, and a ſot by inclination, qualities which commonly furniſh thick blood, the active principles of which are hard to be diſengaged; reaſon proves it, and experience confirms it every day. This being ſuppoſed, it remains to examine the manner in which grief acts. Grief is a diſeaſe of the mind, one of the moſt terrible and moſt fatal, rage, deſpair, fear, revenge, and melancholy, are its uſual effects. What diſorders do not paſſions of this nature produce in the machine! Some precipitate the motions of the ſpirits without order, whence phrenſies ariſe, and an infinite number of acute diſeaſes, others retard the courſe of it, and therefore produce hypochondrical affections, and the greateſt part of chronical diſeaſes. The grief of our ſleeper is of the laſt ſort: at the news of his enemy being killed, he is ſeized with terror, and fills himſelf with melancholy ideas; fear and ſadneſs retain his ſpirits in the brain, his blood naturally thick and deprived, if I may uſe the expreſſion, of the *primum mobile*, thickens more and more, its parts draw cloſer, hang together and entangle the ſpirits; hours of reſt are no longer ſufficient, but whole months are requiſite to ſeparate a quantity of it neceſſary for waking. In this reſpect I am not afraid of comparing him to the marmotte: being thus aſleep he is its true image. This animal, heavy by its natural conſtitution and dull, abounds with fat during the time of its ſleeping; it takes no nouriſhment in its ſix months ſleep; the ſpirits diſengage themſelves inſenſibly by the motion alone of the circulation of the blood, and the reſpiration which it preſerves: at the end of this time it awakes without any help: the ſix months that it is awake, it eats reaſonably, exhauſts but little, its blood becomes of the ſame ſort, and it ſleeps again. Perhaps from the ſame principles, and the ſame reaſoning, we might explain in a more probable manner the changes which happened to our patient during his ſleeping; the two firſt months his ſleep was profound; his blood in all appearance had acquired the quality of the blood of the marmotte, the other two months without opening his eyes, or ſpeaking; he however by intervals, gave ſome ſigns of ſenſe. By the exact diet that he obſerved, the ſpirits diſengaged themſelves, and a greater quantity of them were ſeparated; the marmotte requires ſix months, nature has ſo ordered in forming it; here it is an accident, and may be repaired in leſs time. We have a proof of it, and our patient grows better every day; it now remains to ſeek for the examples which may have relation to it. Neither the ancient nor modern authors furniſh us with any. M. *Homberg* read to the company in the year 1707, the extract of a *Dutch* letter printed at *Goude*, containing the hiſtory of an extraordinary lethargy; it deſerves to be ſet down here as a parallel; grief was the occaſion of it; the ſleeping was preceded by a melancholy affection of three months. For the length of time the *Dutch* ſleeper exceeds the *French* one, he ſlept ſix months ſucceſſively without interruption, and during this time gave no ſigns of voluntary motion, nor of ſenſe; at the end of ſix months he awaked and diſcourſed with every body; and 24 hours after he returned to ſleep again; perhaps he may be aſleep ſtill, for we have not the reſt of this hiſtory. The carpenter in queſtion, in four months ſickneſs, had only two of real ſleep, but the cataleptic accident, the ſigns which he preſerved of a man aſleep, thoſe which he gave of a man awake, the effects that followed the bath of cold-water, are ſo many rare particularities, which render the fact worthy of the attention of the moſt learned philoſophers and phyſicians; my deſign was to have entered into a particular explication of all theſe accidents, but the fear of being troubleſome makes me defer it to another diſſertation.

A
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AN

ABRIDGMENT

OF THE

PHILOSOPHICAL DISCOVERIES and OBSERVATIONS in the HISTORY of the ROYAL ACADEMY of SCIENCES at *Paris*, for the year 1714.

I. *On the paſſage of air and water through certain bodies.*

WE are commonly perſuaded, that water, though more groſs than air, penetrates certain bodies, as paper, which air does not. But perhaps alſo it penetrates them for this very reaſon, that it is more groſs; that is, that it has power to make paſſages which air cannot; perhaps alſo air penetrates the ſame bodies as water, but without being perceived; for it is very dangerous in philoſophy to ſuppoſe facts which are not ſtrictly proved, and we are generally miſtaken in them.

M. *de Reaumur* has thought of a very ſimple and infallible method of being ſure of theſe facts and all others of the ſame nature. The quickſilver keeps ſuſpended in the barometer, only becauſe the tube is ſo exactly cloſed at its upper extremity, that no air can enter. If any ſhould enter, the quickſilver would immediately fall in proportion to what got in, and if it entered by little and little, the quickſilver would alſo fall by degrees, till at laſt it was quite upon a level. If inſtead of air it was water that entered into the barometer, the quickſilver would ſtill fall according to the weight of this quantity of water. We know how much the quickſilver ought to fall for a determinate quantity of both, and reciprocally by the quantity of its falling we know how much of either got in. But if both entered, the water being viſible, we know how much air muſt have got in to make the total effect. It is evident that we muſt always conſider how much the quickſilver ought to fall independantly on the entrance of the air, or water, by the ſole variation which would happen to the barometer at the time of theſe experiments.

This being ſuppoſed, we need only cloſe, as M. *de Reaumur* has done, that extremity of the barometer, which is to be uppermoſt, with the matter which is to be examined whether it is penetrable by air; if the quickſilver ſinks in the tube independantly on the diminution of the weight of the atmoſphere, we ſhall be ſure that ſome air has got in, which muſt have penetrated the ſubſtance which ſtopped the end of the tube. And this ſort of proof has this advantage, that the air acting only by its gravity againſt what ſtops the tube, and this gravity being a known force, equal to 28 inches of quickſilver, we know it is this entire force which at firſt cauſes the air to enter, ſince the top of the tube is perfectly void, and incloſes no air that reſiſts the outer air. If the air continues to enter, we know by the quantity which is already entered, and which is known by the falling of the quickſilver, how much the force which drove it is diminiſhed, and if the quickſilver at laſt comes to a level, we ſee that the leaſt force is capable of making the air paſs through the body under examination. We ſee alſo, which is very conſiderable, what are the different times, in which this force continually decreaſing has occaſion to act according to its different decreaſings. But if the queſtion is about water inſtead of air, or about both together, M. *de Reaumur* has contrived to make with a certain compoſition, impenetrable by air, a little rim raiſed above the tube, by means of which he has a little veſſel, into which he pours the quantity of water deſired, and the ſame reaſonings follow.

I ſhall now mention the reſult of the experiments which he has made upon theſe principles.

Air paſſes through paper even the thickeſt, but leſs quick.

Let the impelling force be ever ſo little, it paſſes but more ſlowly.

If paper be ever ſo little wetted, the air does not paſs through, it begins to paſs again as ſoon as the paper is dry; If we would have it continue wet, it muſt be rubbed with oil.

Air paſſes pretty freely through old parchment. It does not paſs when it is wet.

It is known that water penetrates the bladders of ſeveral animals from without inwards, and not from within outwards. Air ſeldom penetrates a hog's bladder, when its inner ſurface is expoſed to it; and when it does penetrate it, it is with an extreme ſlowneſs, eſpecially when it is impelled but by a ſmall force.

Air does not penetrate a hog's bladder by its outward ſurface, but water penetrates it, tho' pretty ſlowly.

There paſſes with the water a very ſmall quantity of air, which proves both that water may paſs where air does not, and that when it does paſs, it is by opening itſelf paſſages, thro' which air takes the advantage of accompanying it.

Water, tho' impelled only by a little force, penetrates the inner ſurface of the bladder, which it does not penetrate in the living animal, but this is becauſe it is not then impelled by any force, ſince theſe two ſurfaces are equally preſſed by the inward and outward air: Thence M. *de Reaumur* concludes, that membranes of our body, which in their natural ſtate are not penetrable by certain liquors, will become ſo when an extraordinary rarefaction of air ſhall cauſe them to be leſs compreſſed by one of their ſurfaces than by the other. We ſee by this example, that M. *de Reaumur*'s experiments, which might naturally be uſeful in the arts, will be ſo alſo in medicine, and this unforeſeen uſe may give us reaſon to expect more.

II. *Of ſome extraordinary effects of thunder.*

The Chevalier *de Louville*, being at *Nevers*, obſerved ſome remarkable effects of a violent ſtorm of thunder there; a tree of the park of the caſtle, was ſplit into three at the top of the trunk, and had three furrows of unequal bigneſs made in the wood, juſt as if three musket balls had been ſhot from the top of the tree towards the root; the tree was barked on one ſide from about the middle quite to the bottom; tho' it was crooked, yet the three ſtrokes followed the bendings exactly, ſliding continually between the wood and the bark, both in the upper part of the trunk which was ſtill covered with bark, and in the lower part which was covered but on one ſide; but what was moſt remarkable was, that the wood was not blackened at all, and had no ſign of burning.

Upon this, the chevalier *de Louville* explains how thunder can have great effects without burning; it is certain in the firſt place, that it has great ones upon animals, and that when it falls pretty near them, the ſulphurous vapour alone is ſufficient to take away their breath and kill them, with out the appearance of any hurt or bruiſe upon their whole body. But as for the tree, there muſt be another cauſe; M. *de Louville* thinks that when the thunder falls from ſuch a height, that its flame is diſſipated before it can reach the earth, the air being violently driven by the impetuous motion of this flame, and conſequently condenſed in an extraordinary manner, becomes a ſort of hard body, of which the ſhock muſt have a great deal of force.

Hence alſo he explains another clap of thunder, the effects of which he ſaw at *Nevers*, the ſame day. There was a faggot upon the two dogs laid in the chimney in order to be kindled, the thunder fell down the chimney and broke the faggot into a hundred thouſand bits, without ſetting fire to it, or ſo much as blackening it; probably the funnel of the chimney confining the courſe of the air, had alſo augmented the impetuoſity of it.

On this occaſion it was ſaid, that the kindled matter which forms the thunder, may come out of the cloud in a ſmall quantity, and afterwards find in the air a great deal of matter of the ſame nature and kindle it, for then the air is extremely loaded with ſulphurous exhalations. Perhaps this is partly the reaſon why lightning crinkles, for it is ſeeking in the air for a nouriſhment which is irregularly diſperſed.

III. *Of ſome petrified ſhells found at a conſiderable diſtance from the ſea.*

M *de Lagny* has ſeen in *Poitou* ſome petrified ſhells very well preſerved, which were found 8 or 10 feet deep in the ground, upon ſome hills 10 or 12 leagues from the ſea; there were among theſe ſhells ſeveral *cornua ammonis*. He ſaw alſo a field quite covered with oiſter ſhells as large as plates.

IV. *Of the* American *rat*, *or* mus alpinus.

M. *Sarraſin* phyſician at *Quebec*, from whom we have had an exact and curious hiſtory of the beaver in the memoirs of * 1704, has ſent one of the *American rat*, which is very like that which Mr. *Ray* has deſcribed under the name of *mus alpinus*. It alſo ſo much reſembles the beaver, that M. *Sarraſin*, who knows them very well, ſays he ſhould take it at firſt ſight for a beaver of three or four months. That which he diſſected weighed four pounds; we ſhall not enter into the anatomical particulars.

This rat is of the claſs of animals that gnaw. In *March*, when the ſnow that always falls abundantly in *North America*, is not entirely melted, it goes out and lives upon pieces of wood that it breaks; after the melting of the ſnow, it commonly lives upon the roots of nettles, afterwards upon the ſtalks and leaves of that plant, and in ſummer upon ſtrawberries and raſpberries, its nouriſhment grows continually more delicate. A little while after its going abroad it thinks of multiplying its ſpecies; they go freely together 'till autumn, and at the beginning of winter they ſeparate they were ſet again in the free air; theſe have a very ſmall diameter, as about $\frac{1}{3}$ of a line: now what force muſt that be which puts them in motion again as ſoon as they are in the free air?

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* Vol. II. Page 181 of this abridgment.

When they were firſt there they emitted the water drop by drop, and at the diſtances of about 2 ſeconds, whereas the others of a greater diameter emit it in continued threads of an equal diameter to that of the ſecond branch. This difference comes from the very ſmall ſyphons being full of water as ſoon as they are wetted, in their inner ſurface, a drop of water which wets a ſmall part of this ſurface, joins itſelf to the drop of water which is overagainſt it, and this by a certain viſcidity, which naturaliſts acknowledge to be in water. When theſe ſyphons are in the free air, and are once moiſtened by the water, which has paſſed through, that their motion may be continued, the weight of the air muſt ſurmount not only the weight of the water which it has raiſed, but the viſcidity of it alſo, which is performed only by a certain quantity of water collected, and conſequently with a certain time, and thence it comes that theſe ſyphons flow only drop by drop and at ſeveral times. Each drop that comes out partly falls, becauſe it is impelled by the weight of the upper drops. When we put theſe ſyphons *in vacuo*, not only the weight of the air acts continually leſs and leſs, and at laſt acts no longer, but alſo the air contained in the water is extended, becauſe it is no longer preſſed by the outward air; it diſengages itſelf from within the water, and forms great bubbles, which interrupt the *ſeries* of the drops of water, with which the two branches were moiſtened and filled; and thoſe which are at the extremity of the ſecond, have no longer a ſufficient weight, and are no longer ſufficiently preſſed by the others to make them fall. If we replace the ſyphons in the free air, the air which was extended is obliged to reſume its firſt bulk, the drops of water which it no longer keeps ſeparate fall again, the upper upon the lower ones, and the ſyphon begins to flow again as faſt as it is wetted, but always drop by drop, and always more ſlowly, and does not ceaſe 'till its ſecond branch is dry, at leaſt to a certain point.

It follows from this explication, that if water was without air incloſed in its interſtices a very ſmall ſyphon would continue to flow *in vacuo*, as long as it was wet. This is alſo what M. *Homberg* has experimented with water freed from air, whether becauſe it had been made to boil well, or becauſe it had been put into the pneumatic machine, and this *phænomenon*, which ſeems at firſt ſight ſo contrary to the ſyſtem of the weight of the air, agrees perfectly with it, and is a neceſſary conſequence of the ſpring of the air ſtretched by its gravity.

It is eaſy to foreſee, that if for the experiment of the capillary ſyphons, we make uſe of liquors which contain more air, or air which diſengages itſelf more eaſily, ſuch as fermented liquors, the ſyphons will ſtop ſooner in the *vacuum*, alſo, all the reſt being equal, they muſt ſtop ſooner in winter than in ſummer, for in winter the air is more diſpoſed to diſengage itſelf, ſince in frozen liquors it is full of great bubbles; we ſhall judge alſo by this experiment, that fat liquors, as oil or milk contain leſs air, or air more engaged, for with theſe liquors the ſyphons do not ſtop in the *vacuum* in any time whatſoever.

VI. *On the greateſt poſſible perfection of machines moved by animals.*

Mechanicks can do nothing more ingenious and more uſeful at the ſame time, than to determine how far they may be uſeful, and in what bounds all the advantages which they promiſe are contained. All poſſible machines are moved either by animals or by fluids, which are made to work inſtead of animals; thoſe of the laſt ſort have already been examined by M. *Parent*, and now he examines thoſe of the firſt.

An effect of a machine can never be greater than the natural and ſimple effect of the power which moves the machine. Thus if the natural effect of the force of a man is to raiſe 24 pound, by going 1000 toiſes in an hour, and of a horſe to raiſe 170 pound, by going 1800 toiſes in an hour, a machine moved by a man or by a horſe could never do more let it be compounded with ever ſo much art, and it would even be much leſs becauſe of the inevitable frictions, but we do not conſider them here. The effect of the machine moved by a man will therefore never be more than the product of 24 pounds, by a 1000 toiſes of velocity in an hour, or the product of 24 by 1000, in what manner ſoever this product is formed by the weight and by its velocity; for it is always the ſame quantity of motion, and thence it follows, that a man going 1000 toiſes in an hour, may raiſe a weight of 24000 pounds, provided this weight goes but one toiſe in the ſame time, and it is the ſame with all the other infinite ways by which the product 24000 may be formed. The mechanical effect therefore has neceſſarily for a limit the natural effect of the power which moves the machine, and indeed it is impoſſible to draw a new force from nothing. If we would have a man going 1000 toiſes in an hour raiſe 24 pound, it is beſt not to uſe any machine, but if we would have him raiſe more than 24 pound, there muſt be one, which preſerving to the man his natural velocity, diminiſhes that of the weight in proportion as it is bigger: the whole comes to this, the different arms of the lever, by which either the power or the weight act in machines, only regulate their velocities, and always repreſent them geometrically.

When therefore we have a machine moved by animals, which raiſes a weight, its effect being the product of the weight by the velocity which the machine gives it, there is nothing more eaſy than to compare this effect with the natural effect of the animals, and thereby to ſee how much it is leſs, for it is always ſo becauſe of the frictions; the more the machinal effect ſhall approach to the natural, the more perfect will the machine be.

If animals draw one or more boats, the obſtacle which they have to overcome is the reſiſtance of the water, the greatneſs of this reſiſtance depends, 1ſt, on the greatneſs of the ſurface which puſhes the water before it. 2d. on the velocity of this ſurface with regard to that of the water, which is called the *reſpective* velocity of the ſurface: If it is moved the ſame way with the water, its reſpective velocity is the exceſs of its velocity above that of the water; if it is contrary to the water, its reſpective velocity is the ſum of its velocity, and of that of the water; if the water has no velocity, as that of a pond, then the reſpective velocity of the ſurface is its proper and abſolute velocity. Now it muſt be remembred, that becauſe we are conſidering a moved fluid, we ought to take the ſquare of the reſpective velocity. 3d. The reſiſtance of the water depends on its weight, or on its maſs, for it is plain that the ſame ſurface moved in the air with the ſame reſpective velocity, would find leſs reſiſtance than in water.

The obſtacle which animals have here to overcome is therefore the product of theſe 3 magnitudes, the weight or maſs of water, the ſurface moved in the water, and the ſquare of its reſpective velocity: M. *Parent* concludes from ſeveral experiments made by skilful mathematicians, that the water of the *Seine* ſtriking againſt a ſurface of a foot ſquare, with a velocity of a foot in a ſecond, has a force of 22 ounces; it now remains to know, what ſhall be the ſurface that the boats ſhall preſent to the water, the difficulty is that their ſurfaces are crooked, and may be differently crooked in each. But M. *Parent* ſhews a very eaſy way of making them all equal to a plain ſurface, I mean the ſurfaces immerged in the water, which muſt puſh it before them or bear its reſiſtance; we need only put upon the end of a poſt in the middle of the river a fixt pulley, over which paſſes a cord, one end of which is faſtened to the boats that we would draw, and the other to a great flat table, plunged in the water by degrees, 'till it receives a ſufficient impreſſion to be driven according to the ſtream, and thereby obliges the boats to move in order to riſe again; it is certain the part of the table immerged in the water, will be a ſurface equal to thoſe of all the boats together, which have the reſiſtance of the water to ſurmount, there will be as many times 22 ounces of force in the water, as ſquare feet in this ſurface.

We ſhall therefore have the reſiſtance of the water expreſſed in a certain number of pounds, acting with a velocity of a foot in a ſecond againſt a ſurface of a certain number of ſquare feet. Here is what the effort of the animals muſt equal in the ſtate of *equilibrium*, here is what they muſt ſuſtain out of the *equilibrium*, here is the reſiſtance they muſt ſurmount to draw the boats, and as they will impreſs a certain velocity upon them, which will augment the reſiſtance of the water, which the animals will have to overcome, it will be the weight of 22 ounces taken as many times as there are ſquare feet in the flat ſurface of the experiment, and multiplied by the ſquare of the reſpective velocity of the boats; and it is plain, that this product can never be greater than the natural effort of the animals, as we have determined.

If it is a running water, its velocity enters into the expreſſion of the reſpective velocity of the boats, whether they go up or down, and as this velocity is determined, that of the *Seine*, for example, to be one foot in a ſecond, there is a determinate quantity which enters into the reſiſtance of the water, and conſequently requires that a certain part of the force of the animals ſhould be determined alſo. All the reſt almoſt is free, that is, we may vary the force of the animals as we will, and the oppoſite charge, which is the ſum of the ſurfaces of the boats immerged in the water. If the charge remaining the ſame the force of the animals diminiſhes, or which is the ſame thing, their number, for we muſt ſuppoſe that they always go with the ſame velocity, or if the force of the animals remaining the ſame, we augment the immerged ſurfaces of the boats, either by augmenting their number or loading them more, it is plain that in theſe two caſes the boats will go more ſlowly. But if the water is ſtill, the reſpective velocity of the boats being no more than their proper velocity, and the determined velocity of the water making no longer a part of it, then all is free, and we may draw as great a number of boats, or as much loaded as we will, with as ſmall a force as we will, on condition that the boats ſhall go very ſlowly, and this is only what is continually found in all mechanicks.

If the animals which draw the boats are applied to a machine fixed upon the ſhoar, as M. *Parent* ſuppoſes, this machine will furniſh the arms of a lever, one of which will belong to the animals, and the other to the oppoſite charge or load, the proportion of which will repreſent that of the velocity of the animals, to the velocity of the boats.

But if inſtead of a machine fixt upon the ſhoar, we make uſe of a machine carried upon the boats themſelves, which can be only a double mill faſtened on the outſide to the two ſides of the firſt boat which will draw the reſt, then the water acting againſt the ſails, or vanns or floats of each mill, and obliging the cord drawn by the animals to turn about a roller, will impreſs on the boats a motion added to that which the animals impreſs on it.

In all caſes whatſoever, M. *Parent*'s theory gives him a ſure mean to find any magnitude whatſoever, that enters into the moving force or the load oppoſed, when the other magnitudes ſhall be given or known. It is only a calculation, but it ſometimes requires art and delicacy in the application, and thence it comes to have its particular beauty.

VII. *Of a new theory of the working of veſſels.*

This year there appeared a new book of M. *Bernoulli*, intitled an eſſay towards a new theory of the working of veſſels, the firſt and only one that has hitherto come out of his hands; for he has contented himſelf with diſperſing either in our memoirs, or in the *Leipſic* acts, different detached pieces, each of which is equal in value to many a great book. The occaſion of this work was the *theory of the working of veſſels* by the Chevalier *Renau*, printed in 1689, when it appeared, M. *Huygens* had a conſiderable difficulty with M. *Renau* on a fundamental point. As theſe ſubjects do not belong to pure geometry, but depend on a very nice mixture of geometry and phyſicks, and beſides as this was quite new, and as M. *Renau* was the firſt who ventured to touch upon it, it was no wonder, that there ſhould ſtill remain ſome difficulties to clear up, or even that the geometricians ſhould be divided about it: and indeed they were divided, for ſome were for M. *Huygens*, others for M. *Renau*, and among theſe laſt was M. *Bernoulli*, who not having ſeen M. *Renau*'s book, judged of the diſpute only as it had been explained to him by the Marquis *de l'Hopital*; a long time afterwards he ſaw the book and altered his opinion, but he ſtill found himſelf in oppoſition to M. *Renau* on another important point, which M. *Huygens* had not taken the pains to conſider, or elſe had been convinced of it, and as the alteration of theſe two points make a different theory from that of M. *Renau*, much leſs ſimple indeed and more embarraſſing, but according to M. *Bernoulli* neceſſary, and by its great very difficulty more inciting to a great geometrician, he reſolved to make a complete work of it. We ſhall give an account of it, as if it was the only one that had been compoſed on this ſubject, and without entring into either ſide of the controverted points, we ſhall only give ſome preliminary or general informations which will facilitate the underſtanding of the book, for in order to a more large diſcuſſion, the book itſelf ought to be conſulted.

Suppoſe a veſſel at reſt with its ſail, which is ſuppoſed to be flat, and continues always ſo notwithſtanding the action of the wind; this veſſel being impelled by the wind in a firſt inſtant, takes at preſent, becauſe of its great bulk, no more than an almoſt infinitely ſmall velocity, and conſequently the water reſiſts it but very little. The wind blows again in a ſecond inſtant, and impreſſes on the veſſel a new velocity, which being added to the firſt makes an accelerated velocity, and the water makes more reſiſtance to this greater velocity.

Laſtly, the velocity of the veſſel is continually accelerated from one inſtant to another, and the reſiſtance of the water continually augments alſo, till this reſiſtance becomes of an equal force to the augmentation of the velocity of the veſſel; then if the wind and water were to be ſuddenly annihilated, and the veſſel conſequently found in the *vacuum*, it would go to infinite according to a right line, with an uniform velocity equal to the laſt degree of acceleration, which it received from the wind in the laſt inſtant. But this laſt degree, to which the reſiſtance of the water is equal, being acquired, the veſſel is not in the *vacuum*, the action of the wind and the reſiſtance of the water always ſubſiſt; however, becauſe this action and reſiſtance are become equal, they mutually deſtroy one another, and the veſſel is in the ſame caſe as if it was in the *vacuum*; it will go therefore thenceforward with an uniform velocity, and the continual action of the wind upon the ſail will only deſtroy the continual reſiſtance of the water. It is in this ſtate of uniform velocity that the motion of the veſſel is conſidered.

That the motion may be effectively uniform, the wind muſt no longer accelerate it; and for this it muſt meet the veſſel, not as flying before it, for it would accelerate its motion; but as being at reſt, and that the wind may always meet the veſſel as being at reſt, tho' it really flies, the velocity of the wind muſt be as infinite with relation to that of the veſſel. This is alſo what M. *Bernoulli* ſuppoſes in his whole theory. It is true, that when a veſſel makes 3 leagues in an hour, whilſt the wind makes 5, the ſuppoſition is very far from the truth; but M. *Bernoulli* reaſons ſafely upon this ſuppoſition, the known errors of ſuppoſition are not errors in geometry.

The force of the wind, which drives the veſſel with an uniform velocity, and the reſiſtance of the water being equal, and deſtroying each other, theſe two forces muſt act one againſt the other in the ſame right line, for otherwiſe they would produce a common effect, and not deſtroy one another.

The force with which the wind acts upon a ſail always ſuppoſed flat, depends upon three things, or magnitude, by which it is produced, as that of every fluid which ſtrikes a plain ſurface.

1. On the magnitude of the ſurface.

2. On the angle of incidence of the fluid on the ſurface:

3. On the velocity of the fluid.

The 1ſt point is clear.

As for the 2d, it is clear that a fluid, which ſtrikes a ſurface obliquely, ſtrikes it only according to the degree of perpendicularity in its direction, that is, according to the ſine of the angle of incidence, and conſequently the impulſe is ſo much ſtronger as the ſine is greater, or the oblique impulſe leſs oblique. Beſides, in proportion as the incidence is more oblique, a leſs quantity of the fluid ſtrikes the ſurface, and we ſhall ſee it plainly by conceiving the incidence infinitely oblique or parallel to the ſurface, for then the ſurface is not ſtruck by any quantity of the fluid; and in the oppoſite caſe, which is that of the perpendicular incidence, it is ſo thro' the whole poſſible quantity of the fluid. It is very eaſy to prove, that the different quantities of water, which anſwer to the different incidences, are as the ſines of the angles of incidence. Whence it follows, *that the* forces of the different impulſes are as the ſquares of theſe ſines.

For the 3d point, every body knows that the different impulſes of a fluid, moved with different velocities, are as the ſquares of theſe velocities; becauſe a fluid moved with more velocity, ſtrikes both with more force, and in the ſame time with a much greater number of parts, and that this greater number is in the ſame *ratio* with a greater velocity.

Therefore the force of the wind upon the ſail is a product of the ſurface of the ſail, by the ſquare of its ſine of incidence, and by the ſquare of its velocity. If we ſuppoſe the wind always the ſame in different caſes, its velocity is no longer to be conſidered. It will be the ſame with the ſail always ſuppoſed the ſame.

The reſiſtance of a water which is not running, as that of the ſea, to a veſſel which moves, is the ſame as the impulſe of the ſame water if it was running, againſt the ſame veſſel at reſt. Thus the reſiſtance of the water is regulated by the impulſe of the wind upon the ſail, though with a a great difference which muſt be obſerved.

A veſſel has a crooked ſurface, compoſed conſequently of an infinite number of plain ſurfaces infinitely ſmall, differently inclined to each other, which cauſes the incidence of the water to be different from each of them; whereas the incidence of the wind is the ſame upon the whole ſail, always ſuppoſed flat. From all the *partial* reſiſtances of the water to each infinitely ſmall ſurface of the crooked veſſel, there is formed a *total* reſiſtance, which may be alſo called mean; and it is this reſiſtance alone that is equal, and directly oppoſite to the action of the wind upon the ſail.

To be able to expreſs it geometrically, we muſt know the curvity of the veſſel, and the geometricians plainly perceive, that we ſhould then fall into integrations that are often impoſſible, and always difficult. M. *Bernoulli* avoids all this difficulty by conſidering at firſt a veſſel, which is only an oblong rectangular parallelipiped, or if you pleaſe, a ſimple parallelogram, which conſequently has but two ſides differently ſtruck by the water.

Theſe principles being eſtabliſhed, and theſe ſuppoſitions made, there are two principle things to be conſidered on the motion of the rectangular veſſel, its courſe and its velocity.

It is plain at firſt, that its courſe, or the direction of its motion, depends on the line, according to which the wind impels the ſail. The wind, which in general is ſuppoſed oblique to the ſail, impels it only according to what perpendicularity the ſail has in its direction and by no means according to its paralleliſm, and conſequently the line according to which the wind impels the ſail, is always perpendicular to it. Thence it follows, that if the ſail is always in the ſame ſituation, or directed according to the ſame diameter of the horizon, the wind, tho' it falls upon the ſail under different angles, will always impel it according to the ſame line, becauſe the perpendicular to the ſail will be always the ſame, or directed to the ſame point of the horizon. Much more will it be ſo, if the incidence of the wind is always the ſame, for the wind blowing according to a certain determined diameter of the horizon, the equality of incidence determines the ſail to be directed according to another diameter of the horizon, which is always the ſame. M. *Bernoulli* calls the perpendicular, according to which the wind impels the ſail, *the line of the moving force.*

If the veſſel was round, the perfect uniformity of its figure would make it find in all its parts an equal reſiſtance of the water, and conſequently it would go according to the line of the moving force, or the perpendicular to the ſail. If the veſſel being rectangular had infinitely more facility to cleave the water according to the line of its *keel*, which would be a right line drawn thro' its centre parallel to the great ſide, than according to the line perpendicular to the keel, it would go according to the line of the keel, and not according to that of the moving force; for it receives from the wind an impreſſion of motion, and becauſe of the reſiſtance of the water ſuppoſed infinite according to the great ſide of the parallelogram, this motion cannot be executed but according to the ſmall ſide, and conſequently will go only according to the line of the keel. But the ſuppoſition was chimerical, and the veſſel which has more facility to cleave the water by the little ſide than by the great one, has not infinitely more of it, and conſequently is found in a mean caſe between the two extremes which we have conſidered; one of which was that of the equal reſiſtance of the water, and the other that of its reſiſtance infinitely unequal. Therefore, if in the 1ſt caſe it followed the line of the moving force, and in the 2d that of the keel, it muſt in the mean caſe follow a mean line, which will be its true courſe.

In the conſtruction of veſſels it is endeavoured to give them ſuch a curvity, that the greater facility they have to cleave the water by the prow, or according to the line of the keel, than by the ſide, may be the greateſt poſſible, and the intention ſhould be, that the courſe be made according to the line of the keel. It is impoſſible however, but that the water muſt reſiſt the prow as well as the ſide, though much leſs, and conſequently the line of the courſe turns aſide more or leſs from that of the keel, according to the greater or leſs inequality that there is between the reſiſtance that the water makes to the prow, and that which it makes to the ſide, which comes from the different figure of the veſſel. The angle which the courſe makes with the keel is called the *Angle of the Derivation.*

It ſeems at firſt, that the magnitude of this angle muſt depend only on the greater or leſs inequality of the reſiſtance of the water, or which comes to the ſame thing, on the figure of the veſſel. But M. *Bernoulli* makes another conſideration to enter into it. The figure of the veſſel being determined, the line of the moving force may be pends only on the incidence of the wind upon the ſail, the nearer this incidence approaches to perpendicularity, the wind has the more force, and the veſſel the greater velocity.

But if we ſuppoſe the incidence of the wind upon the ſail always the ſame, and conſequently the moving force equal, there is ſtill one thing which makes the velocity of the veſſel vary, the poſition of the ſail with regard to the wind being determined and fixed, that of the ſail with regard to the keel is not ſo, and we may by means of the rudder, put the keel into different ſituations with regard to the ſail, the courſe is a mean line between that of the moving force; which in the preſent ſuppoſition is fixt, and that of the keel. We have already ſeen, in ſpeaking of the angle of *derivation*, that according as the line of the moving force was more or leſs approaching to that of the keel, the veſſel was impelled in a manner more or leſs favourable to its motion, and this is the ſecond condition on which the velocity depends; we ſhall now ſee this more particularly.

The courſe is a mean line between the line of the moving force and that of the keel, and it changes its direction as we have ſeen, when the angle of theſe 2 extreme lines changes. Therefore the poſition of the line of the moving force being fixed, if that of the keel changes, the courſe changes alſo. It is on the direction of the courſe that the different incidence of the water on the great ſide depends, and on the little one, of the rectangular veſſel that is, that according to the different courſe the angles of incidence of the water upon each of the two ſides vary between themſelves, and are more or leſs unequal. Theſe two angles taken together always make a right one, but the ſines of two angles that are equivalent to a right one, do not make a ſum equal to that of the ſines of 2 other angles, which are alſo equivalent to a right one, and conſequently according to the different courſe of the veſſel, the ſums of the ſines of the angles of incidence of the water upon the two ſides are different. Now theſe ſines or their ſquares expreſs the different reſiſtances of the water to each ſide, and conſequently the ſum of the two *lateral* reſiſtances of the water varies according to the different courſe, and conſequently alſo the velocity of the veſſel. It is plain that a leſs velocity anſwers to a greater ſum of the lateral reſiſtances.

The two lateral or partial reſiſtances of each courſe produce a mean or total reſiſtance, which it is eaſy to calculate. Each of theſe mean reſiſtances is equal to the force by which the veſſel is impelled. Now in the two different courſes, the moving force is the ſame, ſince the incidence of the wind upon the ſail is ſuppoſed the ſame; therefore the two mean reſiſtances are equal, and as there neceſſarily enters into the expreſſion of each, the particular velocity, by which the veſſel is impelled according to each courſe, we have the relation of theſe two different velocities to magnitudes, into which there enter only the known ſides of the veſſel, and the courſes or lines which depend upon it.

From all that has been ſaid it reſults, that there muſt be *a greater* for the velocity of the veſſel, that is, that the incidence of the wind upon the ſail being always the ſame, there will be a poſition of the keel with regard to the line of the moving force, ſo that the veſſel will have a greater velocity in this diſpoſition than in any other. This greater velocity, which the calculation gives without difficulty, is ſound when the diagonal of the rectangular veſſel is in the line of the moving force, and conſequently perpendicular to the ſail; one might be ſurpriſed at firſt, that it is this diagonal, which ſhould be put in this ſituation, and not the keel, that is, the line drawn through the centre of the parallelogram, parallel to the great ſide, for the keel is always the moſt favourable line to the motion. But it is eaſy to ſee that the greater velocity anſwers to the leaſt reſiſtance of the water, and that it is at the point, or at any angle whatſoever of the parallelogram that the water reſiſts the leaſt; what had been called the keel was not ſo in reality, it is the diagonal that is ſo, and the error of the word being diſſipated, there is no longer any paradox.

The line which expreſſes each velocity, is always taken upon the line of the courſe, and as the different velocities of the veſſel for each courſe, or which comes to the ſame thing, for each poſition of the keel, have known relations, we may conceive the infinity of lines which will expreſs the velocities as drawn, and forming by their extremities a curve, which M. *Bernoulli* calls *the determiner of the velocities*. He ſhews how to draw it by points, that is, for all the different poſitions of the keel, the angle of the wind with the ſail being always the ſame.

This being done, we ſee that unleſs the line of the courſe does not touch the curve, which only one courſe can do, it cuts it in two points, and as all the points of this curve determine the velocities, there are therefore, except in one ſingle caſe, two different velocities for one ſingle courſe; and becauſe each point of the curve anſwers to a different poſition of the keel, we may make the ſame courſe with 2 different poſitions of the keel, but with 2 different velocities. It is very eaſy to ſee which of the two is the greateſt, and we muſt chuſe that, and conſequently give the keel the poſition which anſwers to it.

There is a conſiderable advantage which they often endeavour to procure by the working, and that is *gaining upon the wind*, as a wind which blows from eaſt to weſt, for example, and falls obliquely upon the ſail, impells it only according to a perpendicular, it does not impel the veſſel to the weſt, and the perpendicular may be ſuch as ſhall be directed according to the line of the S. E. and conſequently its direction will partake of a direction from W. to E. and laſtly the impulſe of the wind ſuppoſed will tend to make it go partly from W. to E. and even againſt the wind. Now it is an advantage to go againſt the wind, or to gain upon the wind, not only when we would take a courſe, to which it is almoſt entirely contrary, but when we would board a veſſel, or avoid being boarded by it. M. *Bernoulli*'s curve ſhews alſo what is the courſe that we muſt hold, or the poſition that we muſt give the keel, to gain upon the wind as much as poſſible.

If the courſe that we would hold is determined, which is the moſt uſual caſe, we find by means of the curve, that 2 different poſitions of the keel are equally proper for it, and we ought to chuſe that which gives the greateſt velocity.

All that has now been ſaid ſuppoſes the angle of incidence of the wind upon the ſail conſtant, or to ſpeak more exactly, the line of the moving force always directed to the ſame point of the horizon, which, as we have ſeen, ſubſiſts as long as the ſail is directed according to the ſame diameter of the horizon, whether it receives the wind under different angles or under the ſame; but if the poſition of the ſail changes, which makes the direction of the line of the moving force change, or of the perpendicular to the ſail, then there would anſwer to this new poſition of the ſail another poſition of the keel, with regard thereto, to give to the veſſel the greateſt poſſible velocity in this new hypotheſis. And in like manner all the infinitely different poſitions of the ſail will have their different poſitions of keel correſponding to a greater velocity.

If we had determined neither a poſition of the ſail according to a certain diameter of the horizon, nor a poſition of keel with regard to the ſail, and had ſought to determine theſe two poſitions together, to have the greateſt poſſible velocity, this greater velocity would be therefore the greateſt among the infinite number of greateſt, each of which reſults from a certain determinate poſition of ſail, and from the moſt advantageous poſition of the keel correſponding thereto. The determination of this *greateſt of the greateſt*, is one of the moſt elevated geometrical problems. M. *Bernoulli* reſolves it, and is led by this reſolution to important remarks, the occaſion of which is rare. But we cannot enter either into this inquiry, or theſe reflections. M. *Bernoulli* himſelf prefers another method for the practice; he calls it mechanical, and yet it depends on a ſublime geometry.

If for each different poſition of the ſail the determining curve of the velocities was deſcribed, there ſhould be an infinity of theſe curves, ſince there may be an infinity of poſitions of ſail infinitely little different, and this infinity of curves, the firſt and ſecond, the ſecond and third, *&c.* would in-

interſect each other ſucceſſively in points infinitely near. The ſeries of this infinity of points of interſection would form a new curve which would touch all the *determiners of the velocities*, and would contain all their conditions and advantages. M. *Bernoulli* ſuppoſes it, not exactly, which is impoſſible in practice, but ſufficiently well deſcribed; and he ſhews how to find by means thereof, what will be the poſition both of the ſail and of the keel, to make a courſe propoſed with the greateſt poſſible velocity, or to gain the moſt upon the wind.

Hitherto the figure of a parallelepiped or parallelogram, which has been given to the veſſel, has been very different from that which is uſually given them. To come ſtill nearer, M. *Bernoulli* takes a veſſel which has the figure of a rhomb, or loſenge, but the ſame principles always ſubſiſt, only the explications of it become more difficult, and the calculations more complicated. Laſtly, after having paſſed thro' theſe degrees, M. *Bernoulli* comes to a curvilineal figure. It is in general any portion of a curve, which has an *axis*, and ordinates.

The curve which repreſents the figure of the veſſel cannot be ſtruck by the water, but according to the line of the courſe, or which is the ſame thing, parallelly to this line. The parallels to the courſe drawn upon all the infinitely ſmall ſides of the curve are different incidences to it, but each of them ſtrikes only according to a perpendicular to a little ſide that it ſtrikes. Any one of theſe perpendiculars to the curve being drawn or conceived to repreſent all the reſt, it is compoſed of two directions, one of which is perpendicular, and the other parallel to the *axis* of the curve.

Thence it follows, that each little ſide is ſtruck or impelled according to two directions, one perpendicular, and the other parallel to this *axis*. If we have the proportion of the ſum of all the impulſes perpendicular to the *axis*, to the ſum of all the parallel impulſes, a rectangle, of which the two ſides ſhall have this proportion being deſcribed, the diagonal will repreſent the impulſe or mean reſiſtance of the water, and the poſition of this diagonal will be neceſſarily oblique to the *axis* of the curve. We know by all that has been ſaid, that the poſition of this line of the mean reſiſtance of the water is always the ſame as that of the line of the moving force.

If the incidence of the water upon all the little ſides of the curve changes, there are formed two other ſums of impulſions or reſiſtances, perpendicular and parallel to the *axis* of the curve; theſe ſums have another proportion, and conſequently the diagonal of the ſuppoſed parallelogram, or the line of the mean reſiſtance of the water, changes in magnitude and poſition, and the line of the moving force changes alſo in poſition. Now the incidence of the water on all the ſmall ſides of the curve neceſſarily changes, as ſoon as the veſſel changes its courſe, which is viſible; therefore the change of the courſe makes the poſition of the moving line change. It is reciprocally plain, that if the poſition of the line of the moving force changes, that of the courſe changes alſo. The poſition of the line of the moving force gives that of the ſail, ſince it is always perpendicular to it.

Theſe two lines, that of the moving force and that of the courſe being mutually dependent on each other, as to the poſition, one of the two poſitions being given, the other therefore may be deduced from it. Let us ſuppoſe at firſt the poſition of the given courſe.

To have that of the line of the moving force, or of the mean reſiſtance of the water, we muſt have the ſums of the lateral reſiſtances of the water, ſome perpendicular, the others parallel to the *axis* of the curve, and conſequently determine the curve. M. *Bernoulli* ſuppoſes the figure of the veſſel to be formed of two equal circular ſegments, having the keel for a common chord, which comes pretty near the ordinary figures. He expreſſes algebraically the differentials of the two ſorts of lateral reſiſtances, aud happily the ſums of theſe are found to be integrable; and conſequently he has their proportion, whence all the reſt follows. This proportion is pretty compound.

It changes, as we ſee it muſt do, at each alteration of the courſe, but there are 2 principal cauſes, which in ſome ſort change the ſpecies of it. There is always one half of the veſſel, which bears the reſiſtance of the water, whilſt the other is intirely covered from it. If the half expoſed to the reſiſtance of the water is one of the two circular ſegments entire; or if this half is compoſed of the 2 equal halves of the two ſegments, it is plain that the reſiſtance of the water is conſiderably different in the 2 extreme caſes, much greater in the firſt, becauſe the veſſel takes no advantage of its pointed figure to cleave the water; and much leſs in the ſecond, becauſe it takes as much advantage of its point as poſſible. Now what determines the half of the veſſel expoſed to the water is the 2 lines parallel to the courſe, the moſt diſtant from it on both ſides that can be drawn through two points of the ſurface of the veſſel. If they paſs through the 2 extremities or points of the veſſel, it is in the firſt caſe, and preſents to the water only one ſingle circular ſegment; if they paſs thro' the 2 extremities of the perpendicular drawn thro' the middle of the keel, the veſſel is in the ſecond caſe, and preſents to the water the half of each circular ſegment, or advance of point.

If the 2 parallels to the courſe paſs thro' the 2 points of the veſſel, or extremities of the keel, and at the ſame time are each of them tangents of their ſegment or circular arch, it follows from thence, that the arch which they touch, or the infinitely ſmall ſide, which is one part of them at the point of contact, makes with the keel the ſame angle as the courſe. But if the courſe, and conſequently the two parallels come to incline more upon the keel, and conſequently to make with it a leſs angle than the fixed angle, and conſiſting of each circular arch with the ſame keel, then the two parallels become tangents of the 2 circular arches in other points, and one of the points of the veſſel begins to be contained in the part expoſed to the water, and is more and more ſo, till at laſt the line of the courſe being infinitely inclined to the keel, or concurring with it, the two parallels are tangents of the veſſel at the two extremities of the perpendicular, which paſſes through the middle of the keel. But if the angle of the courſe with the keel is greater than that of each of the two circular arches with the keel, the two parallels paſs always through the two extremities of the keel, but without being tangents of the veſſel, the part expoſed to the water is always one entire circular ſegment; and laſtly, the courſe may concur with the perpendicular to the keel. Theſe are the two principal caſes which change the ſpecies

of

ſpecies of the proportion between the lateral reſiſtances of the water.

We ſhall find the velocities of the veſſel of a circular figure for different courſes, the moving force being the ſame, as we have found thoſe of the rectangular veſſel. It is the ſame reaſoning.

But if in the veſſel of a circular figure, the poſition of the line of the moving force was determined, and the courſe was to be determined, we might do it; but by an algebraical calculation, ſo long and ſo compound, that M. *Bernoulli* thinks it more proper to ſubſtitute tables, wherein the poſitions of the line of the moving force ſhould anſwer to all the courſes, and the courſes to them. Theſe tables ſhould alſo contain the velocities.

To bring all this to the utmoſt exactneſs, we ſhould determine the figure of the veſſel, which would make it bear the leaſt poſſible reſiſtance from the water. This determination has already been made for a veſſel, the courſe of which ſhould be according to the keel, but it is not in the ſuppoſition of all the other poſſible courſes, and as the reſiſtance of the water will change in all theſe courſes, there muſt be a different figure of a veſſel for each of them: It does not appear therefore, that geometry can aſpire to this enterpriſe, and we muſt hold to the trials of experience, which will nearly determine a mean figure, the moſt ſuitable to all the courſes.

The poſition of the line of the mean reſiſtance of the water with regard to the courſe, or which comes to the ſame thing, with regard to the keel, being determined, this line neceſſarily paſſes through a certain point of the keel. On another ſide the line of the moving force paſſes through the middle of the ſail, and what is a neceſſary conſequence of it, through the maſt to which the ſail is faſtened, or, which comes to the ſame, through the foot of the maſt which always reſts upon the keel, or, laſtly, through a certain point of the keel. If the courſe is ſuch, that the line of the mean reſiſtance of the water, and that of the moving force paſs only thro' the ſame point of the keel, the reſiſtance of the water, which is equally diſtributed on both ſides of the line of the mean reſiſtance, is therefore not equally diſtributed on both ſides of the line of the moving force, and as theſe lines muſt however be one right line, the veſſel will turn till they are both in this poſition. To hinder this turning, we muſt know through what point of the keel, the line of the mean reſiſtance of the water paſſes, and there fix the maſt.

M. *Bernoulli* ſhews how to determine this point. The reſiſtance perpendicular to the *axis* of the curve being expreſs'd algebraically and conceived as ſo many weights faſtened to a lever, which is the *axis*, have a common centre of gravity therein. In like manner the reſiſtances parallel to the *axis* have a common centre of gravity upon a perpendicular to the *axis*; two perpendicular lines both drawn through theſe two centres of gravity, interſect each other in a point, which is the centre of gravity of the two ſpecies of reſiſtances; the line of mean reſiſtance therefore muſt paſs through this point, which conſequently determines the point where it cuts the keel, and where the maſt ſhould be fixed.

But as the line of the mean reſiſtance changes for every courſe, the point where the maſt ſhould be planted would change alſo, which being impoſſible in practice, we can only find for the maſt pretty nearly a mean ſituation, the moſt commodious for all the different courſes.

It remains only to rectify the ſuppoſition of the ſail always flat, it is very certain that it is not, and that by the action of the wind it takes a certain curvity, thereby the line of the moving force changes, and it is a perpendicular to the curve of the ſail, ſuch that on both ſides the impulſes of the wind on the differently inclined parts of the ſail are equal, or which is the ſame, that it is its *axis* of *equilibrium*, as the line of the mean reſiſtance of the water is the *axis* of *equilibrium* of all the particular reſiſtances. We muſt therefore determine the curvity of the ſail, or which is the ſame problem more ſimple, the curvity of a ſimple cord, which ſhould be in the ſame circumſtances as the ſail, and for this it is neceſſary to eſtabliſh two principles, one of which has been already in this diſcourſe.

1ſt. A homogeneous and perfectly flexible cord being ſtretched, it is equally ſo in all its parts, what figure ſoever it takes by the tenſion; for if we conceived one part to be more ſtretched, as it would make an effort to reſume its firſt ſtate, it would draw to it a leſs ſtretched part, which would yield to it, and conſequently both would come to an equal tenſion.

2. A cord, which in its different parts receives oblique impulſes or preſſures, is impelled or preſſed only according to the perpendiculars to the points of incidence, and theſe perpendiculars are the ſines of the angles of incidence.

When a cord bends becauſe it is drawn by weights or puſhed by a fluid which acts upon it, it is ſtretched or impelled at the ſame time and by the ſame forces, but is equally ſtretched and unequally impelled or preſſed. And to ſee the difference of theſe two actions of tenſion or preſſure ſtill better, we need only obſerve, that a cord or any part whatſoever of a cord, is ſtretched only ſo far as it is drawn according to its length or direction, and that on the contrary it is preſſed only ſo far as it is preſſed perpendicularly to this length.

The tenſions make no alterations by themſelves in the direction of a part of a cord, but the preſſures change it, ſince they neceſſarily impel this part according to a certain perpendicular.

When a cord bends, its only becauſe each of its infinitely ſmall parts, which becomes a ſide of the curve, receives the action of a power different from that which has acted on the preceding ſide, or which ſhall act on the following. Thereby each little ſide takes a different direction from thoſe of the two ſides which contain it. If a cord was at the ſame time unequally ſtretched, and unequally preſſed, we ought to conceive ſome ſide longer, becauſe of the inequality of tenſion, and at the ſame time of a direction more or leſs different from that of the neighbouring ſide, becauſe of the inequality of preſſure. Now I ſuppoſe it is known, that the greatneſs of the bending of a curve is in a direct *ratio* of the greatneſs of the change of direction from one ſide to the other, or of the angle of contingence and in a reverſed *ratio* of the magnitude of the ſides into which we conceive it to be divided. Therefore if the cord was unequally ſtretched and preſſed, it would take a curvity which would be in a *ratio* compounded of the reverſed *ratio* of the tenſions, and of the direct one of the preſſures. But the cord is always equally ſtretched, therefore its curvity is only in a direct *ratio* of the preſſures, and there is the general determination of all theſe ſorts of curves. As the *radii* of the evolutes are always in a reverſed *ratio* of the curvatures, we muſt take theſe curves ſo that their *radii* of the evolute at each point may be in a reverſed *ratio* of the preſſure at the ſame point. Therefore we have only to determine what ſhall be the different ſpecies of preſſure.

If we conſider, as we do here, a ſail ſwoln by the wind, the preſſures will be as the ſquares of the ſines of the angles of incidence of the wind upon each little ſide of the curve. This is plain from all that has been ſaid; we ſuppoſe that each thread of wind gives its ſtroak to the ſail, after which it goes freely out of its cavity; otherwiſe it would make in this cavity different reflections, which would change the primitive impulſes, and conſequently the nature of the curve.

If the ſail contained an elaſtic ſubſtance, as air, the ſpring of which acts equally every way, it is plain that the preſſures being all equal, the curve ſought would be a circle.

If we ſuppoſed a linnen bent by a heavy liquor, which it ſhould contain, the preſſures would be the heights of the different columns of this liquor.

We muſt not forget in this place that M. *Bernoulli* finds again what he had formerly demonſtrated, that the curve of the ſail is the ſame with the *catenaria*, a curve formed by a cord faſtened at its two extremities to two fixt points, and bearing at all its points, of which the number is infinite, as many equal weights. It ſeems that by following the reaſonings which have been made, one may eaſily prove the identity of the two curves.

The *catenaria* is equally ſtretched in all its parts, and conſequently it will be the ſame as the curve of the ſail, if the preſſures on both ſides follow the ſame proportion. The preſſures of the *catenaria*, are drawings of weights, all the directions of which are vertical and parallel. The direction of each weight is oblique to each little ſide of the *catenaria* and conſequently muſt be decompounded into perpendicular and parallel to the *axis* of the curve, which is the line drawn from one of the fixt points to the other. The perpendicular to the axis is the only line, according to which the weight draws the ſide, it is the ſine of the acute angle, which is made by the vertical direction of the weight with the ſide, and, if we may ſo ſpeak, of the angle of incidence of the weight. The greater this perpendicular to the *axis*, or this ſine is in proportion to what parallelism there is in the direction of the weight, the more it acts or draws with force.

It is eaſy to ſee, that by taking the curve from one of the fixt points to its middle, the ſines of the angles made by the directions of the weights with the ſides are continually increaſing; for all the weights being ſuppoſed equal, their actions or drawings can be only as their diſtances from this fixt point, which continually augments, but their actions are as theſe ſines, therefore the ſines continually augment from a fixed point, where the origin of the curve is, quite to its middle.

This being ſuppoſed, each ſide of the curve is ſo much the more drawn, not only as each weight acts by a greater ſine, but alſo as each weight acts with more advantage with regard to a neighbouring ſide, for to draw the cord downward, it is ſufficient for a weight to have any force whatſoever, but to bend it actually, it muſt draw it more than another weight draws it, without which all the force poſſible would be of no ſervice. In a word, there muſt be a force in each weight, and an inequality of forces in all, I mean in one half of the curve. Now not only the force of two weights taken ſeparately and by themſelves will be expreſſed by two ſines, ſuch as we have marked, but their inequality, or proportion of forces will be expreſſed by the ſame ſines; whence it follows, that the drawings of theſe two weights will be as the ſquares of theſe ſines, which is exactly the proportion of the preſſures of the wind in the curve of the ſail. Therefore the curve of the ſail and the *catenaria* are the ſame thing.

In the application of M. *Bernoulli*'s theory to practice, there is a very conſiderable advantage, that to determine the poſition of the *axis* of *equilibrium* of the impulſes of the wind, or of the line of the moving force upon the bent ſail, we are not obliged to know the nature of the curve of the ſail, and to calculate it, which would be very difficult. M. *Bernoulli* demonſtrates, that if from the middle of each of the two ſides, one above, the other below, which terminates the bent ſail, we draw a tangent; the line, which ſhall biſect the angle of the concurrence of the tangents, will be the *axis* of *equilibrium* of the impulſes of the wind.

An Explanation of the Terms *of* Art *uſed in this Volume, which were not explained at the End of the former Volumes.*

C.

C*Repuſculum* the *Twilight*.

E.

Empalement of a flower is that part which covers the *Petals*, or coloured leaves. In a roſe, the five green leaves on the outſide of the flower, is the *Empalement*.

I.

Iris is the variegated circle, which ſurrounds the pupil of the eye.

L.

Lamina ſpiralis is a thin plate, which divides the *cochlea*, or laſt cavity of the ear into two parts. That part, which is next the *axis*,

axis, is bony, but extremly brittle; and that next the outer ſhell is membranous, appearing to be only made of the auditory nerve.

M.

Membrana tympani, is a thin tranſparent membrane, which cloſes the inner extremity of the *meatus auditorius*. It is of an oval figure, ſtretched out like the head of a drum; it makes an obtuſe angle with the upper and back part of the *meatus*, and an acute one with the lower and fore part.

S.

Summit or *apex* is uſed to expreſs thoſe little buttons, which are ſuſtained by the *chives* or *ſtamina* in flowers. They are moſt commonly of a yellow colour, and contain a fine golden duſt, which is thought to be analogous to the male ſperm of animals.

Syzygia or *ſyzygy*, is uſed to expreſs either the conjunction of a planet with the ſun, or its oppoſition to it.

T.

Terras of the obſervatory at *Paris* is a platform on the top of that famous ſtructure. It is paved with flints laid in mortar, cement, and lime.

Tympanum is a pretty large cavity in the ear, ſituated behind the *membrana tympani*; it is about 3 or 4 lines deep, as many wide, and between 2 or 3 high. It is lined with a fine membrane, on which there are ſeveral veins, and arteries. In this cavity are contained 4 ſmall bones, called the *malleolus*, the *incus*, the *ſtapes*, and the *os orbiculare*.

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CONTINUATION *of the* ADDENDA.

In the MEMOIRS *of* 1701.

X. *Of the meridian of the royal obſervatory, prolonged to the* Pyreneans; *by M.* Caſſini*.

The journey, that we made this year by order of the king into the ſouthern provinces of his kingdom, was to prolong the meridian line of the royal obſervatory, to meaſure it quite to the extremities of *France* by geometrical operations; to divide it into degrees of the circumference of the earth by the obſervations of the ſtars; to examine whether theſe degrees are equal between themſelves as has been ſuppoſed till this age, or whether they are ſenſibly unequal, as ſeveral modern mathematicians ſuppoſe; whether the perpendicular lines marked by the plummet in our geometrical and aſtronomical inſtruments, are all directed to the ſame point, which is taken for the centre of the earth, according to the common *hypotheſis*, or whether from different places, which are at a great diſtance from each other, they are directed to points ſenſibly different, as ſeveral moderns conjecture.

Theſe laſt inquiries ought to have been made before it was undertaken to calculate the whole circuit of the earth by a ſmall part. For unleſs theſe two laſt *hypotheſes* of the roundneſs of the earth, and of the direction of the perpendicular to the ſame centre are well eſtabliſhed, there is no certainty in the practice of this method. Thus *Pliny* had ſome reaſon to admire at the boldneſs of the human mind, in attempting ſuch difficult things. The enterprize alone of examining theſe two *hypotheſes* by obſervations is great, and would be raſh without the protection of ſo great a king. The *hypotheſis* of the roundneſs of the earth, as compoſed of continents and ſeas, that of its detachment from the heavens, and of its ſuſpenſion in the air, was firſt of all founded upon the obſervation of the apparent motion of all the ſtars from eaſt to weſt, and on the diverſity of the apparent conſtitution of the heavens, in the voyages made almoſt under the ſame meridian towards the ſouth and towards the north. This diverſity compared with the length of the way gave the firſt views of meaſuring the circumference of the earth by the obſervations of the ſtars. The oldeſt author now extant, who has related the particulars of this method, is *Ariſtotle*, who ſpeaks of it as a thing already in practice in his time.

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* November 14, 1701.

He expreſſes himſelf to the following purpoſe, at the end of his ſecond book *de cælo*.

"It is manifeſt, ſays he, by the appearance of "the ſtars, that not only the earth is round, but "that it is not of any immenſe bigneſs; for if we "travel ever ſo little towards the ſouth and north, "the horizon varies, and the vertical ſtars, which "we have over our heads, make a great change, "and are not all the ſame that we ſee going to the "north and ſouth. For there are ſome which we "ſee in *Egypt*, and near *Cyprus*, which we do not "ſee in the northern countries, and towards the "north there are ſome ſtars which we always ſee

" above ground, tho' they ſet in the places juſt " mentioned. Wherefore thoſe who think that " the ſea, which is at the pillars of *Hercules*, unites " and makes the ſame ſea with that which is at the " *Indies*, do not imagine improbabilites.

" The mathematicians, adds he, who endea- " vour, to calculate the magnitude of the circum- " ference of the earth, make it amount to 400,000 " *ſtadia*. Whence it is inferred, not only that " the earth is ſpherical, but that it is not very " great with regard to the other ſtars.

This muſt have been the language of the authors of this dimenſion. For *Ariſtotle* far from ſuppoſing the earth to be a ſtar, in the preceding chapter refutes the *Pythagoreans* of *Italy*, who reckoned the earth in the number of the ſtars, and aſcribed to it a motion about the centre of the world, ſo as to make the alternative of days and nights; which they would not have done, if they had not ſuppoſed the earth to be nearly of the ſame figure and bigneſs with the ſtars. This dimenſiòn related by *Ariſtotle* might therefore be aſcribed to the *Pythagoreans*, who were the authors of this *hypotheſis*. It is almoſt twice as big as it was afterwards found by other mathematicians. But it did not appear large enough at a time when there were yet ſome philoſophers, who, after *Xenophanes*, queſſtioned whether it was not of an immenſe magnitude.

Immenſum ne patent terræ ima, et largior æther.

The appearances of the ſtars related by *Ariſtotle*, ſuggeſt two methods of undertaking the meaſure of the earth, which were practiſed in the following ages:One by the obſervations of the ſtars ſituated vertically in one place, and at a diſtance from that ſituation in another; the other by the obſervation of the ſtars in the horizon of one place, and elevated above the horizon of another.

Eratoſthenes, under king *Ptolomy Euergetes*, put the firſt method in practice; he knew that at the time of the ſummer ſolſtice the ſun paſſed over the vertical point of the city of *Syene*, ſituated in the confines of *Ethiopia* under the tropic of *Cancer*. There was a well made for this obſervation, which on the day of the ſolſtice at noon was quite illuminated by the ſun, and it was notorious that at 150 *ſtadia* round it, the ſtyles raiſed perpendicularly on a horizontal ſurface made no ſhadow; having ſuppoſed *Alexandria* and *Syene* to lie under the ſame meridian, he obſerved at *Alexandria* on the day of the ſolſtice the diſtance of the ſun at the vertical point, by the ſhadow of a ſtyle raiſed perpendicularly from the bottom of a concave hemiſphere; he found that this diſtance was the 50th part of the circumference of a great circle; whence he concluded, that the diſtance between theſe two cities was the 50th part of the circumference of the earth. Having computed this diſtance to be 5000 *ſtadia*, he had the whole circumference 250,000 *ſtadia*.

Having divided it equally into 360 degrees, he had 694 and almoſt $\frac{4}{9}$ to the degree. But afterwards, inſtead thereof he took the round number 700, not thinking perhaps that it would anſwer to 5 or 6 *ſtadia* in a degree.

And multiplying 700 *ſtadia* by 360 degrees, he had in the circumference 252,000 *ſtadia*, which is the laſt dimenſion of *Eratoſthenes*, which he commonly made uſe of. *Hipparchus* alſo made uſe of it, tho he judged that 2520 *ſtadia* ought to be added to this dimenſion.

Dionoſidorus only took for the ſemidiameter of the earth $\frac{1}{6}$ part of its circumference drawn from the laſt dimenſion of *Eratoſthenes*, in the letter which was found in his tomb after his death, to make it believed that he had gone down to the centre of the earth, and that having meaſured the diſtance he had found it 42,000 *ſtadia.*

Vitruvius and *Pliny* reduce the meaſure of *Eratoſthenes* of 252,000 *ſtadia*, to 31,500 *Roman* miles, allowing 8 *ſtadia* for a mile.

Poſſidonius, in the time of *Pompey* the great, undertook to meaſure the circumference of the earth by the ſecond method, that is, by the horizontal obſervations.

He learned that at *Rhodes* the ſtar *Canopus* did but juſt appear at the horizon, and ſet immediately; and that at *Alexandria*, which he ſuppoſed to be under the ſame meridian, it roſe above the horizon $\frac{1}{48}$ part of the circumference of the heavens, which anſwers to a like part of the circumference of the earth of 7° $\frac{1}{2}$, and ſuppoſing the diſtance between theſe 2 cities to be 5000 *ſtadia*, he had the whole circumference of the earth 240,000 *ſtadia.* This is the firſt dimenſion of *Poſſidonius*, related by *Cleomedes*, an author of the ſame age, adding that it muſt be diminiſhed, if the interval (in *ſtadia*) is not found to be quite ſo great.

Strabo, who wrote his geography under *Auguſtus* and *Tiberius*, aſcribes to *Poſſidonius* the dimenſion of the circumference of the earth of 180,000 *ſtadia*, which are in the proportion of 500 *ſtadia* to the degree, it was not eaſy to diſcover the foundation of this; but here it is. The ſame author teſtifies in another place, that *Eratoſthenes* had meaſured the diſtance between *Rhodes* and *Alexandria* by inſtruments, and found it to be 3750 *ſtadia*, taking it for $\frac{1}{48}$ part of the circumference of the earth according to *Poſſidonius*, there reſult 180,000 *ſtadia.* It may therefore be called the laſt dimenſion of *Poſſidonius*, in which were uſed his dimenſions in degrees, and that of *Eratoſthenes* in *ſtadia*. It was received by *Marinus Tyrius* the geographer and others. It is commonly aſcribed to *Ptolomy*, becauſe he made uſe of it in his geography. We ſhall not here relate what is oppoſed to theſe methods, we ſhall only obſerve, as a thing of ſome conſequence, that having taken exactly the mean between the laſt dimenſions of *Eratoſthenes* and of *Poſſidonius*, we have in a degree of the circumference of the earth 600 *ſtadia*, in a minute 10 *ſtadia*, which by the account of *Pliny* and *Vitruvius* make 1 mile $\frac{1}{4}$ of the ancient *Roman* meaſure. Now the modern *Italian* mile is equal to 1 mile $\frac{1}{4}$ of the ancient miles, the diſtance of 25 miles, which the ancients reckoned between *Bolonia* and *Modena* being eſteemed at preſent to be 20 modern miles. The modern *Italian* mile is 10 *ſtadia*, which make a minute, according to the mean dimenſion between that of *Eratoſthenes* and *Poſſidonius*. The degree of the circumference of the earth will be therefore by this account 60 modern *Italian* miles, and 75 ancient miles. The circumference 21,600 modern miles, 27,000 ancient miles.

Allowing to the mean league 3 ancient miles, we ſhall have 25 leagues in a degree, and in the whole circumference 9000 leagues.

After *Eratoſthenes* and *Poſſidonius*, ſeveral have made uſe of the heights of the pole, in the meaſuring of the earth. The mathematicians of the Caliph *Almamon* having taken the heights of the pole in the plains of *Singar*, at the extremities of two degrees, found 56 miles in one degree, and 56 miles $\frac{2}{3}$ in the other, and judged their meaſure to be leſs than that of *Ptolemy* by 10 miles. This is very different from all the other dimenſions, which make it much greater. The geographer of *Nubia*, an author of the 12th century, allows 25 leagues to the degree; this dimenſion was confirmed by that of *Fernellius*, this author made uſe alſo of the heights of the pole taken from the obſervations of the ſun, to find a place nearly under the meridian of *Paris*, which was a degree diſtant from it, and having meaſured the diſtance of it by the wheel, he found it 56747 toiſes, having abated the turns of it at diſcretion. According to the eſtimation of the people of the country, this diſtance was 25 leagues.

Snellius, who ſurpaſſed in exactneſs thoſe who had gone before him, made uſe of the heights of the pole obſerved at *Alkmaer* and *Bergen op zoom*, differing between themſelves 1° 11′ ½, and having meaſured the diſtances by the triangles, he found in a degree 56,946 toiſes of 6 *Rhinland* feet each, and by the difference between *Aclmaer* and *Leyden* at the diſtance of ½ a degree, he found 57,020 toiſes in a degree; He took for a mean between the two 57,000. M. *Picard* conſidering the difference between the *Rhinland* and *Paris* feet, reduces it to 55,021 toiſes.

Above 42 years ago I made ſeveral attempts at *Bolonia* and at *Ferrara*, to meaſure the earth, both by the vertical and horizontal obſervations, which are related by F. *Riccioli* in his geography. It was only to transfer it to the meridian line, which I drew in the great church of St. *Petronius*, to keep account of it, if neceſſary, in the obſervations of the ſun. At the diſtance of 34 toiſes and 2 feet of *Paris*, where the middle of the image of the ſun at the winter ſolſtice is, I marked the 600,000 part of the circumference of the earth, which is in the proportion of 57,222 toiſes to a degree.

F. *Riccoli* and *Grimaldi* took alſo ſeveral ways and with a great deal of care, the meaſure of the earth, as it is related at length in the *Geographia reformata*. M. *Picard* has reduced it to 64,363 *Paris* toiſes to the degree.

But nothing of this kind has ever been done with more care and exactneſs, than what was executed by M. *Picard* in the name of the royal academy of ſciences, immediately after its inſtitution, in the three firſt years.

He meaſured exactly by triangles the diſtances between the parallels of *Malvoiſine*, *Sourdon*, and *Amiens*, which are but at a little diſtance from the meridian of *Paris*, and he obſerved with an inſtrument of 10 feet, the diſtances of the ſame fixed ſtar from the zenith of theſe 3 places. By the diſtance between *Malvoiſine* and *Sourdon*, he determined the magnitude of a degree to be 57,064 toiſes, and by that between *Malvoiſine* and *Amiens* of 1° 22′ to be 57,052 toiſes. He took for a mean between theſe two 57,060 toiſes. The baſe, which he actually meaſured is 11 times greater than that of *Snellius*. He uſed in the ſame diſtance but a ſmall number of triangles in proportion to the great number, which had been uſed by *Snellius*. He made uſe of inſtruments more finely divided, and furniſhed with teleſcopes, which augment and diminiſh the diſtant objects better, and ſerve to determine the diſtances with a greater exactneſs. With ſeveral helps he made uſe of the fineſt ſeaſons of 3 years. I aſſiſted at ſeveral of theſe geographical and aſtronomical obſervations, which we concerted together.

The meaſure of the magnitude of a degree of the circumference of the earth, eſtabliſhed by the academy, might already ſerve to correct the differences of longitudes hitherto found by too ſhort a meaſure. But it was thought neceſſary to undertake again under the king's protection, to determine the longitudes of particular places by aſtronomical obſervations made at the ſame time at the royal obſervatory, and elſewhere. The eclipſes of the moon, which may ſerve for this purpoſe, being too rare to be able to make any advantage of them in a ſhort time, it was propoſed to make uſe of the obſervations of the eclipſes of the ſatellites of *Jupiter*, of which we had already given tables and *ephimerides* immediately after the inſtitution of the academy. For one eclipſe of the moon there happen above a hundred of one ſingle ſatellite. It was found by experience, that the time of their immerſions into the ſhadow of *Jupiter*, and of their emerſions, may be determined with more exactneſs and evidence, than in the eclipſes of the moon. The royal academy immediately attempted this with ſome ſatisfaction. They at firſt had a correſpondence of theſe obſervations with the academy of *Tuſcany*, which was then flouriſhing, and with the royal ſociety of *England*. There was not an aſtronomer in *Europe*, who was provided with neceſſary inſtruments, that did not bear a part in theſe obſervations, and communicate them to the academy. The king ſent to have obſervations made not only on the coaſts of *France*, but in the moſt conſiderable places of the 4 quarters of the world. Some religious orders, famous for the profeſſion of learning both divine and human, thought of turning theſe obſervations to the ſervice of religion in the kingdoms of the *Eaſt*, where aſtronomy is in reputation. Their obſervations, as well as thoſe of moſt of the aſtronomers of *Europe*, compared with thoſe which were made at the ſame time at the royal obſervatory of *Paris*, have ſerved to determine in a ſhort time the differences of longitude between this and moſt other conſiderable cities.

2d. There was not any thing of more importance than to have a meridian well drawn, as that of *Paris* might be drawn thro' all *France*; to take it for bounds of the eaſtern and weſtern lontudes of all the other places of the earth, as it is the moſt illuſtrious of the univerſe. The geographers differ among themſelves in the bounds of the meridian of *Paris* in the *Mediterranean*, by the whole extent of the country between the eaſtern outlet of the *Rhone*, where *Ptolemy* directs it in his geography, and the city of *Valencia* in *Spain*, thro' which it is drawn by *Hondius* in his map of *Europe*. Was it not neceſſary to clear up a point of ſo great conſequence in geography, and alſo in aſtronomy for the reduction of the aſtronomical tables from one meridian to another, and for the determination of the eclipſes?

This work was begun by the deſcription of the meridian at the obſervatory, both by the obſervations of the ſun at the ſolſtices, and by the fixed ſtars.

It has been ſeveral times obſerved, and after different manners, and a mean has been taken between the differences. The angles have been taken, which the meridian makes with the diſtant places ſeen at the obſervatory, the diſtances of which had already been determined in the firſt dimenſion. Perpendiculars have been drawn from theſe places to the meridian, which determine their diſtance to eaſt and weſt, and cut the portion of the meridian intercepted between the parallels of theſe places, and of the obſervatory. And in calculating the diſtance of thoſe places from each other, and the angle of the diſtance from the meridian, views have been taken to the places moſt advanced to the ſouth, where the ſame obſervations have been made, always forming triangles of the preceding diſtances with the following. Theſe triangles have been continued, without ever interrupting the *ſeries*, to the ſouthern extremity of the *kingdom*. When there did not preſent any remarkable objects proper for the continuation of the triangles, there were trees or pyramids elevated, or other marks viſible at a diſtance in the places which were thought the moſt proper for this purpoſe.

In this manner the meridian was drawn from *Paris* to the higheſt mountains of the *Pyrenæans*, which ſeparate *Rouſſillon* from *Catalonia*. The principal towns which are neareſt the meridian of *Paris*, where the celeſtial obſervations have been made, are *Orleans*, *Aubigny*, *Bourges*, *Aubuſſon*, *Aurillac*, *Rhodes*, *Alby*, and *Carcaſſone*, which is exactly in the meridian of *Paris*: *Perpignan* and *Colioure* were found much nearer the eaſt than was ſuppoſed. We have not neglected the difference between the lines drawn thro' the high mountains, and the arches of the circumference of the earth which anſwer to them.

In theſe higheſt mountains, where it was neceſſary to go up, the height of the quickſilver in the barometer was obſerved, to compare it with the height obſerved at the ſame time at *Paris*, and to determine the heights by the compariſon with the obſervations made elſewhere upon mountains, the height of which above the ſurface of the ſea had been meaſured by levelling. The ſame heights have been alſo obſerved by means of the diſtances from one mountain to another, and of their apparent heights taken by inſtruments, continuing theſe obſervations to the ſea. The higheſt of all theſe mountains is the *Canigou*, which is generally covered with ſnow: there had been elevated upon it in the proper ſeaſon, by order of the king, a ſort of pyramid, which made it known at a diſtance. Its true height, taken from the apparent at the ſea-ſide, and from its diſtance, was found to be 1440 toiſes, which are at leaſt 15 *ſtadia*. *Cleomedes* ſays, there is not any mountain, of which the perpendicular height exceeds 15 *ſtadia*; this mountain therefore, in the judgment of that author, paſſes for one of the higheſt.

We ſhall not relate here the obſervations which we have made of the great irregularities of the direction of the needle in the mountains of *Limouſin*, *Auvergne*, and *Rouergue*, abounding in mines of iron. We muſt not be ſurpriſed if the maps of theſe provinces are ill laid down. They cannot be otherwiſe, if they are drawn by the compaſs, as is commonly done.

We have made ſome obſervations of the heights of the pole about our meridian, about all the principal towns, where we made any ſtay in a favourable ſeaſon, and particularly in the places determined in our triangles, to compare the differences of the latitudes found in the heavens with theſe dimenſions taken upon the earth. Our dimenſions upon the earth have been confirmed by a baſe of 7200 toiſes actually meaſured, and by the continuation of the triangles quite to the ſea-ſide where we took it.

For fear of parting too far from the meridian in ſo long a way, we examined it by new obſervations in the places where we were able to do it conveniently, and we have not found any ſenſible difference in it greater than what is uſually found, drawing it ſeveral times by different obſervations in the ſame place.

We have had a correſpondence of obſervations of the eclipſes of the ſatellites of *Jupiter* made at *Paris*, *Lyons*, *Marſeilles*, at the ſame time that we made them about the meridian line, and we have found that the difference of the meridians determined by theſe obſervations agreed with thoſe which we had found by the triangles, which have ſerved to draw the meridian nearly in the terms as thoſe which are made in the ſame place by different obſervers agree together. We have made the ſame uſe of the eclipſe of the moon of *February* 22, 1701.

And as the difference of the meridians between *Sette* and *Montpellier* had been determined by the obſervations of the ſatellites of *Jupiter*, which were made by M. *Picard* in thoſe places, compared with thoſe which we have made at the ſame time at *Paris*, we have been willing to find the difference of theſe meridians by the continuation of the triangles to thoſe places, and calculating their diſtances from our meridian, and reducing it into minutes and ſeconds, we have found it alſo to agree with thoſe which we had determined by the ſatellites of *Jupiter*.

To have the degrees of the circumference of the earth between the parallel of the obſervatory and the bound of our meaſure in the *Pyreneans*, we made uſe at *Colioure* of an inſtrument of 10 feet *radius*, to obſerve ſeveral fixt ſtars near the *Zenith*, and to compare them with thoſe which remained for us to obſerve at *Paris*, by the ſame inſtrument the next year, in the ſame ſeaſon. In the mean time we made uſe of thoſe which had been taken at *Paris* by the common inſtruments; and we found it to be 6° 18′.

In the hypotheſis of the equality of the degrees of the meridian drawn upon the ſurface of the earth, we ſhould have for each minute 954 toiſes, 5 feet, 2 inches 5 lines, and for each degree 57,292 toiſes. But there are ſome very famous mathematicians who queſtion this hypotheſis, as well as that of the direction of the perpendiculars to the ſame point. Since the globe of *Jupiter* has appeared to us a little oval, it has been queſtioned whether the earth was not alſo a little oval, and after the obſervations made by the royal academy, at *Cayenne*, *Cape-Verd*, and the *American* iſlands, have informed us that the pendulums of the ſame length make ſlower vibrations near the equinoctial than at a greater diſtance; M. *Huygens* and Sir *Iſaac Newton* have endeavoured to explain this *phænomenon* by a hypotheſis, which makes the equinoctial greater than the meridians, ſo that its figure would be flatted at the poles; whereas M. *Eiſenſchmid*, by comparing the different dimenſions of the degrees made in different places, to make them agree together, has ſuppoſed the earth to be oval, longer from one pole to the other than according to the diameter of the equinoctial, making the meridians of an elliptical figure, of which the perpendiculars are directed to different points of the *axis*. He waited however for the ſucceſs of our dimenſions, to be able to judge of it with more certainty.

The magnitude of the minute, which we have juſt found in the *hypotheſis* of the equality, in the interval between *Paris* and the parallel of *St.*

St. Elme, exceeds that which M. *Picard* found in the ſame *hypotheſis* in the interval between *Malvoiſine* and *Sordon*, which was 951 toiſes $\frac{1}{15}$, and ſtill more that which is drawn from the obſervations made at *Sourdon*, and *Amiens*, which make the minute 947 toiſes. The exceſs of ours above the firſt, is almoſt 4 toiſes, 5 feet; above the ſecond, 7 toiſes, and almoſt 5 feet: thus the further we recede from the equinoctial, the more the dimenſions upon the earth diminiſh, which ſeems favourable to the modern *hypotheſes*, which we have juſt related, it is difficult to find any one that makes theſe differences agree ſufficiently together.

It appears however by the compariſons which we have made, that in the ſpace between the parallels of *Amiens* and *Colioure*, which is 7° and above $\frac{1}{3}$, the meaſure of the degrees going toward the equinoctial increaſes, ſo that the following degree exceeds the preceding by its 800th part, which in this interval varies from 70 toiſes and 2 feet, to 72 toiſes, and 1 foot. The degree taken from the obſervatory towards the north gives us 57,055 toiſes, and the degree taken from the obſervatory towards the ſouth gives 57,126 toiſes $\frac{1}{2}$. By this progreſſion the magnitude of the other degrees will be found till that which it continues uniformly. The minute from the obſervatory towards the north is 951 toiſes, 5 feet $\frac{1}{2}$. It is equal to the diſtance between the obſervatory and the church of *St. Severin*, and may paſs for the ſmalleſt diameter of *Paris*. Toward the ſouth, the minute from the obſervatory is 951 toiſes, 5 feet, 7 inches $\frac{1}{2}$, and the augmentation of 1 inch, $\frac{1}{3}\frac{1}{5}$, in each minute amounts to 72 toiſes from one degree to another.

We have been ſurpriſed to ſee that this augmentation of $\frac{1}{800}$th part from one degree to another, at this diſtance from the poles, agrees with the augmentation of the degrees of the true diſtance of the moon in its *apogeum* from the ſame degrees of diſtance between 40 and 48 degrees, from the pole on one ſide, and the *apogeum* of the moon on the other. For in this place the degrees of the true diſtance of the moon in its *apogeum* increaſe alſo from one to another $\frac{1}{800}$th part. Thus the perpendicular lines, which terminate the degrees meaſured in the heavens, would be analogous to the lines of the mean longitudes of the moon, and the arches of the circumference of the earth between theſe perpendiculars would be of the ſame magnitude as the arches of the ſame circumference contained between the lines correſponding with thoſe of the mean motion of the moon.

If this equality, which is found in theſe 7 degrees of the circumference of the earth, of which we have the dimenſions, is found in the whole circumference; it is what we dare not advance, tho' the moon has ſome ſhare in the *equilibrium* of the ſea, to which it brings ſome variation evident in the flux and reflux regulated by its motions. It would be a fact to be verified by dimenſions of a greater extent, if other princes would contribute as much as the king to the perfection of the ſciences.

FINIS.